MIND

A QUARTERLY REVIEW

OF

PSYCHOLOGY AND PHILOSOPHY

I.—CARNAP'S THEORIES OF TRUTH

By D. R. Cousin

§ 1. *Introduction*

In his *Introduction to Semantics*,[1] Carnap makes the claim that
the new science of semantics will ' be of great importance for the
so-called theory of knowledge and the methodology of mathema-
tics and of empirical science ' (p. viii). The claim is based in part
on the use of semantical methods by Tarski in the construction
of a theory of truth.[2] And according to Carnap himself ' it turns
out that truth . . . ' is a concept ' based on the relation of
designation ', and hence a semantical concept (p. 10). Com-
menting on what is laid down by Tarski as a requirement which
must be satisfied by an adequate definition of truth, Carnap adds
the following :—

"The requirement mentioned is not meant as a new theory
or conception of truth. Kotarbinski has already remarked that
it is the old classical conception which dates back to Aristotle.
The new feature is only the more precise formulation of the
requirement. Tarski says further that the characterisation
given is also in agreement with the ordinary use of the word
'true '. It seems to me that he is right in this assertion, at
least as far as the use in science, in judicial proceedings, in
discussions of everyday life on theoretical questions is concerned.
But I will not stress this point ; it may be remarked that Arne
Ness has expressed some doubts about the assertion, based on

[1] Harvard University Press, 1942. Page and section references in this
article, unless otherwise specified, are to this work.
[2] Tarski, *Der Wahrheitsbegriff in den Formalisierten Sprachen*, Lwow,
1935.

systematic questioning of people. At any rate, this question is of a pragmatical (historical, psychological) nature and has not much bearing on the questions of the method and results of semantics " (p. 29).

Ignoring the qualifications expressed in the last two sentences, we gather that Carnap is offering philosophers and others concerned with the so-called theory of knowledge the help of the new science of semantics. The prospect is held out that by the use of semantical methods solutions of familiar problems may be reached or at least approached. At the very least, we are offered more precise formulation of old solutions, carrying with it no doubt some increase of clarity. In particular, we are offered this help in the elucidation of the classical conception of truth, or—what is evidently taken to be the same thing— we are offered a characterisation which is in agreement with the ordinary use of the word ' true '. We seem to be invited to notice something about this ordinary use which has in the past (I suppose) been insufficiently attended to—namely that truth is a semantical concept.

The value of Tarski's contribution has been discussed by Max Black.[1] But Carnap's treatment presents certain peculiarities which perhaps justify further discussion. In what follows, I put (inter alia) the questions : What does Carnap mean by calling truth a semantical concept ? Is his characterisation in accordance with the ordinary use of the word ' true ' ? If not, can we find a characterisation of truth which is in accordance with the ordinary use of the word ' true ', and what kind of concept should we say that it is ?

Accepting the assumption that the problem of truth can be approached by asking questions about the ordinary use of the word ' true ', I propose to test Carnap's proposals by comparison with a set of specimens which I hope will be admitted to exemplify this use. I do not dispute the lack of precision detected by Carnap in ordinary language (p. 241). But I hope to suggest that Carnap's proposals are in an important way *not* in accordance with ordinary usage. I admit the possibility that the question whether the characterisation is in accordance with ordinary usage or not may have ' not much bearing on the questions of the methods and results of semantics ' (p. 29, quoted above). But in case it has not, the methods and results of semantics have perhaps not much bearing on the philosophical problem about truth.

[1] "The Semantic Definition of Truth," *Analysis*, 8, 4, March, 1948.

The following specimens are intended to recall the sort of sentence in which the word ' true ' normally occurs. (I confine myself to the English word, but it would not surprise me if closely similar specimens could be found in a number of other languages.) If my specimens seem trite or far-fetched, make your own collection : but, if you make them up, accept only such as a plain man might actually use in conversation. (In the *Strand Magazine* ? Yes, if you like.)

(A) *Queen Anne is dead.*
(B) *Yes, that's true.*
(C) *What did he say ?*
(D) *That Queen Anne was dead.*
(E) *Is that true ?*
(F) *Is what true ?*
(G) *What he said just now—that Queen Anne is dead.*
(H) *Oh yes ; it's true that she's dead.*
(I) *What he says is always true.*
(Z) *Die Königin Anne ist tot.*

It will be convenient and in accordance with Carnap's practice to refer to these specimens by name. In what follows, accordingly ' (A) ' is to be read as a *name* (not an abbreviation) of the sentence ' Queen Anne is dead ', and similarly for the others.

I shall comment on some of these specimens later. Meanwhile, I hope it will be granted that (B), (E), (F), (H), and (I) exemplify fairly well the sort of sentence in which a speaker of normal English uses the word ' true '. Let us call them ' standard T-sentences ', or—for short—simply T-sentences. Notice that in these—and I believe in all or most ordinary examples—the word ' true ' occurs in construction with a pronoun or phrase representing another sentence, in this case the sentence ' Queen Anne is dead '. Let us call this sentence (which sometimes, *e.g.* in (H), appears—in the form of a that-clause—in the T-sentence itself) its ' R-sentence '—the sentence to which the T-sentence, in some way to be discussed later, refers.

I propose to consider only very simple T- and R- sentences. No doubt ' true ' is ambiguous, meaning different things in more and in less complex sentences. But I assume that this ambiguity is systematic. Hence I hope it will *turn out* that what I say applies also, *mutatis mutandis*, to more complicated sentences ; *i.e.*, that it will be only a matter of logic to extract from what is said about simple cases what ought to be said about the others.

Two further preliminary points :

(a) In view of the stress laid, in the passages quoted from

Carnap, on the *semantical* character of truth, it is a little surprising to find that in the *Introduction to Semantics*, while the term ' true ' is applied chiefly to sentences it ' may also be applied in an analogous way to propositions as designata of sentences ' (p. 26). In this case ' no reference to a language is made ; the concept is not a semantical but an *absolute* concept' (§17, p. 88. Carnap's italics). ' True ' (it ' turns out ') is one of a group of terms which can be used—with an ambiguity which need not mislead because it is systematic—in accordance either with a semantical or with an absolute concept. Others are ' false ', ' equivalent ', ' disjunct ', ' implicate ', etc., etc. These absolute concepts do not belong to semantics, and their definitions need not take the roundabout way through semantics (p. 90). Indeed, it would appear that Carnap regards absolute concepts as not ' in any of the fields of semiotic ' (p. 89). Truth, then, so far from being semantical, is from this point of view not even a syntactical, a pragmatical or in any sense a semiotical concept.[1]

If we call the theory that truth is a semantical concept the ' Semantical ' Theory of truth, what are we to say of the theory that truth is not a semantical but an absolute concept, or at least that the word ' true ' may be used in accordance with an absolute *as well as* in accordance with a semantical concept ? Shall we say that Carnap recognises an 'Absolute' Theory of truth as well as a semantical one ? Since this appears to be the case, it seems advisable to investigate this absolute or non-semantical theory : what is its relation to the Semantical Theory ? and how far is it a satisfactory theory on its own merits ? These topics are accordingly discussed in § 4 below.

(*b*) Consider, next, Carnap's informal explanation of ' the way in which the term " true " is used in these discussions '. ' We use the term here ' (he explains) ' in such a sense that to assert that a sentence is true means the same as to assert the sentence itself ; *e.g.*, the two statements " The sentence ' The moon is round ' is true " and " The moon is round " are merely two different formulations of the same assertion (p. 26). Highly semantical, no doubt ; but surely not very idiomatic ? Do we, in English, assert sentences ? Do we not rather *utter* sentences in asserting *propositions* ? And would it not normally be a proposition rather than a sentence, that we should assert to be true ? Perhaps this is pedantry : but it is ordinary usage that

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[1] For the meaning of these terms see *Introduction to Semantics*, § 4 ; also Morris, C. W., " Foundations of the Theory of Signs," *International Encyclopedia of Unified Science*, Vol. I, No. 2, Chicago, 1938.

we are trying to understand and to use as a clue to the elucidation of the ' concept ' of truth.

Further, does this explanation apply only to the semantical concept of truth ? Or does it hold also for the absolute concept ? In the case of absolute terms there is at least a similar ' requirement of adequacy ' (p. 90); and though this is formulated in a way which involves a reference to language it is doubtful whether this is necessary. At any rate, I can see no reason why, if the absolute concept of truth can be defined ' in a straightforward and rather simple way ' (p. 90), it should not be possible to explain this use without reference to language. Why should not we say, *e.g.* : to assert that a proposition is true means the same as to assert the proposition itself ? This seems to me a perfectly straightforward and natural thing to say. It is in line with the theory first publicly formulated, so far as I know, by F. P. Ramsey [1] and now standard among those who follow Wittgenstein. But it does not seem to me to be semantical, and I do not think that Carnap would say that, in this form at any rate, it is semantical. If this is correct, Carnap's informal explanations—as is perhaps to be expected—do not enable us to decide between the alternative views on the relative positions of the semantical and the absolute concepts of truth which have been canvassed above. Nevertheless mention of this theory, which I propose to call the ' Identity ' Theory of truth, will have had its value if it raises in our minds the further question : Is Carnap's account of the semantical, or of the absolute, concept of truth consistent with his informal explanation of the requirements which a theory of truth must satisfy—with the Identity Theory ?

§2. *The Semantical Theory of Truth*

In accordance with the Semantical Concept of truth, the term ' true ' is applied to sentences. The statement is made (p. 26) in explanation of Carnap's own practice in his semantical studies. But it expresses, I take it, the characteristic tenet of the Semantical Theory of truth. That it is to sentences, rather than to any other linguistic expressions, that the term is applied goes, it seems, without saying. Nevertheless the central position occupied by sentences in the field of semantics, and indeed of semiotics, surely deserves more comment and explanation—perhaps even more reflexion—than Carnap appears to have devoted to it. Is it not a very remarkable fact that in the entire course of two books, one—*Logical Syntax of Language* (Original,

[1] *Foundations of Mathematics* (1927), p. 143.

Vienna, 1934 ; London and New York, 1937)—decidedly long and the other—*Introduction to Semantics*—sizable and provided with elaborate apparatus designed (and successfully designed) to secure lucidity, Carnap never finds it worth while to mention any *reason* why they both deal incessantly with sentences, to the almost complete exclusion of other kinds of linguistic expression ? The cat utters a muffled miaow in *Meaning and Necessity* (Chicago, 1947) : ' Only (declarative) sentences have a (designative) meaning in the strictest sense, a meaning of the highest degree of independence. All other expressions derive what meaning they have from the way in which they contribute to the meaning of the sentences in which they occur ' (p. 7). If this animal had been allowed a little scamper much much earlier, who knows whether Ryle would have had to write so scoldingly —or indeed at all—in a recent issue of a contemporary ? (*Philosophy*, Vol. xxiv, No. 88, January, 1949.)

This, however, is not the point I wish to dispute ; if we are to apply the term ' true ' to linguistic expressions of any sort, by all means let it be to sentences. The question which engages me is rather whether the term ' true ' ought to be, or normally is, applied to linguistic expressions at all.

For the sake of definiteness, let us see how the theory looks when we express it as a statement (or recommendation) about T-sentences, illustrated by reference to our specimens. The statement or recommendation is that T-sentences are (or ought to be) *about* some other sentences, *viz.*, their R-sentences. More formally, we might say that the grammatical subject of a T-sentence is (or ought to be) always the name of a sentence, a description of a sentence, or a pronoun standing for a name or description of a sentence.

How does this work out in application to our examples ? In (B), the pronoun ' that ' is not to be understood as replacing or abbreviating the sentence ' Queen Anne is dead ' (A), but as *referring* to it, *i.e.*, as replacing its *name*. Thus we might expand (B) as follows : ' (A) is true ' ; or ' " Queen Anne is dead " is true ', where ' " " Queen Anne is dead " " ' is taken as a name for the sentence ' Queen Anne is dead ', just like ' (A) '. A similar account applies to (E). In (G), the grammatical subject is ' what he said just now ', and this is to be read as a *description* of (A), *i.e.*, of the sentence ' Queen Anne is dead '. In (I), the grammatical subject ' what he says ' is presumably the designation of a class of sentences, *viz.*, all sentences uttered by that particular speaker.

What are we to say about (H) ? If we are to hold that the

concept of truth here applied is the semantical concept, we shall have to read the words 'that Queen Anne is dead' as equivalent to a name for (A), *i.e.*, as equivalent to placing the words 'Queen Anne is dead' within quotation-marks. Perhaps Tarski would be hardy enough to carry his theory so far as this. But Carnap, it seems, would not. Instead, he would admit that the concept of truth here employed is not the semantical concept but the absolute one (p. 241). In this case, accordingly, the grammatical subject of the sentence is to be taken as equivalent not to the *name of the sentence* (A) but to the *designation of the proposition* which (A) states, or expresses, or (in Carnap's own technical terms) 'designates'; *i.e.*, it is equivalent to the sentence (A) itself. What possesses the property designated by 'true', then, is not in this case a sentence but a proposition, *viz.*, the proposition that Queen Anne is dead, an actual or possible state of affairs (p. 235). (Carnap recognises that the word 'proposition' is sometimes used in a way equivalent or analogous to 'sentence', but prefers to apply it to the states of affairs which sentences mean.)

It is characteristic of the Semantical Theory, then, to restrict the occurrence of the word 'true' to sentences the grammatical subject of which names or otherwise refers to some sentence, *viz.* the R-sentence. Where the word 'true' occurs in a sentence the reference of which is *not* to an R-sentence, it is to be regarded as used in accordance with some other concept, presumably the Absolute Concept, of truth.

So much for the grammatical subjects of T-sentences. What of the word 'true' itself? On the face of it, and before we consider the implications of his definitions, Carnap's practice suggests that it is to be treated as a one-place predicate; a T-sentence is complete when we have placed the words 'is true' to the right of the name of the R-sentence (or its equivalent). That he would accept this account of the syntax of T-sentences is suggested by his contrasting those semantical concepts which are themselves relations between expressions of a language system S and entities in the realm of designata of expressions of S (*e.g.*, designation itself and some others), and 'semantical concepts of a second kind [which] although based on relations of the kind just mentioned, are themselves attributed only to expressions, not to designata', among which he includes the concept of truth (pp. 88, 89).

This, then, is the syntactical convention which (according to the Semantical Theory of truth) governs or ought to govern the use of the word 'true'. Can we now go on to say something

about the *semantics* of the word ? We have seen that the grammatical subjects of T-sentences are to be regarded as naming or designating (R-) sentences : what does ' true ' itself name or designate ? (I ask the question in this way because it seems appropriate to Carnap's theory. I do not wish to be understood as endorsing the underlying assumptions.) On the face of it a one-place predicate ought to designate a property, and it will be the characteristic feature of the Semantical Theory of truth that this property should be a semantical property, *i.e.* one possessed only by signs—specifically, by sentences. (This is the fact which is stated in another way when it is laid down that the occurrence of the word ' true ' is restricted to sentences about sentences.) But since the defining peculiarity of sentences is taken to be a *relation* (*viz.* the relation called by Carnap 'designation ') in which sounds or marks on paper stand to something else, we must be prepared to find that the function of ' true ' is crypto-relational—that a full statement of what it means will involve some reference to the relation in which the R-sentence stands to something, and also to the correlate of the R-sentence in respect of this relation. As Carnap says, the concept of truth, though attributed only to expressions, is ' based on ' the semantical relation of designation (p. 89).

(An analogy may help. If we were to say about Queen Anne that she was married, without saying anything about Prince George, then ' married ' might appear to be a one-place predicate. But in fact, since you can't be married without having a spouse, it is relational. If so used to conceal the fact that it is relational, it is crypto-relational.)

I do not think that it is necessary for the present purpose to follow Carnap into the complex technicalities of his attempt to find an adequate, exact definition for the concept of truth. If I understand it correctly, his theory may be indicated roughly as follows. According to him, everyone who utters a T-sentence makes two assertions : (1) the assertion that the R-sentence designates something ; (2) the same assertion which is made by the R-sentence.

Is the first of these assertions to be thought of as general or specific ? Does (B), for example, (*a*) express the assertion that (A) has *some* meaning, without specifying what ? Or does it (*b*) express the assertion that (A) means that Queen Anne is dead ? Probably it is not necessary to decide : both cases are possible, according as (B) is spoken by someone who (*b*) does, or (*a*) does not, understand (A). The important thing is that the second assertion must have a corresponding character : the man whose

assertion (1) is that (A) means that Queen Anne is dead will assert (2) that Queen Anne is dead; the man whose assertion (1) is merely that (A) has *some* meaning will merely commit himself to *some* assertion (2), but not to any assertion in particular. (Or he may be understood as committing himself to the assertion of what any, or some particular, speaker of (A) asserts, although he does not know what this is. A state of affairs deplorable indeed but surely far from uncommon.)

I hope that the foregoing discussion will have brought out the character of the Semantical Theory sufficiently to make intelligible the criticisms to which I will now proceed. Briefly it seems to me that the theory is in accordance neither with ordinary usage nor with the requirement of adequacy as informally laid down by Carnap himself. The second of these objections is perhaps not of great importance, but the first— if it is well-founded—surely undermines the claim of the Semantical Theory to elucidate the ordinary usage of the word ' true '. The alternative remains of course, that the theory though unacceptable as a description of ordinary usage can be read as a recommendation which we should be wise to adopt for the regulation of future usage. But I do not see that it offers any very striking advantages. I will now develop these points further.

A. *That the Semantical Theory is inconsistent with the requirement of adequacy.* According to Carnap's explanation of the way he intends the term ' true ' to be used, a T-sentence is to make the very same assertion as its R-sentence. But how is this possible if it is about the R-sentence ? A T-sentence, as we have seen, mentions or contains the name of its R-sentence ; it says something about it, *viz.* that it designates something, and perhaps even that it designates this and not that. But the R-sentence cannot refer to itself, cannot contain a name or description of itself. The two therefore cannot make precisely the same assertion. Carnap might perhaps reply that his informal explanation must not be taken too literally ; the purpose of his explanation was mainly to draw attention to the *second* assertion made in the T-sentence—the important point about a T-sentence is its making *this* assertion, which is the same as the one made by the R-sentence, and not its making the assertion (1) that the R-sentence designates something. But this reply, if it were made, would surely amount to saying that just that part of a T-sentence which makes it semantical, *viz.* what it asserts about the R-sentence, is unimportant. And this would weaken the claim that the Semantical Theory has important lessons for the philosopher.

B. *That the Semantical Theory is inconsistent with the ordinary usage which it professes to describe.* It does not seem to me that ordinary T-sentences, such as the specimens exhibited above, which I have called (B), (E), (F), (H) and (I), are about their R-sentences, or any other sentences ; it does not seem to me that they mention their R-, or any other, sentences ; or that they contain names, or descriptions, or representatives of names or descriptions, of their R-, or of any other, sentences. It may be difficult to say just what account ought to be given of the expressions which Carnap takes to be names, descriptions, *etc.*, of the R-sentences ; and in the absence of such an account it may not be easy to show convincingly that these ordinary T-sentences are not about their R-sentences. But I think it is not merely certain but obvious that the T-sentences constructed by Carnap in accordance with the Semantical Theory are very far from normal. Adopting this line of approach, I hope to be able to establish my main point.

According to Carnap, a T-sentence and its R-sentence will always be (in some sense) in different languages : whatever the language in which the R-sentence is formulated, a T-sentence must necessarily be formulated in a language of higher level, a ' meta-language ', distinguished from that of the R-sentence (the ' object-language ') as the language in which the object-language is investigated, and therefore as equipped with certain expressions the use of which is to designate expressions of the object-language. If we study English R-sentences in English T-sentences, we must use the meta-part of the English language for the formulation of our T-sentences and other sentences belonging to semiotics (pp. 3, 4). Consider then such a sentence as ' $\mathfrak{S}_1$ is true ', where ' $\mathfrak{S}_1$ ' is the name of the sentence ' The moon has no atmosphere ' (p. 28). It would be trivial no doubt, to object that this cannot be an ordinary English T-sentence, because ordinary English T-sentences do not contain Gothic characters. For it would be easy to call $\mathfrak{S}_1$ by some other name, such as ' John ' or ' Mary '. If we were to object that this would still not be ordinary, because in English ' John ' and ' Mary ' are names of persons and not of sentences, Carnap would no doubt fall back upon the device of writing $\mathfrak{S}_1$ itself in inverted commas. His T-sentence now reads : ' The sentence " The moon has no atmosphere " is true '.[1] Everyone, Carnap seems to suppose, will recognise that ' " The moon has no atmosphere " '

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[1] *Cf.* 'The two statements " The sentence ' The moon is round ' is true " and " The moon is round " are merely two different formulations of the same assertion.' (P. 26.)

CARNAP'S THEORIES OF TRUTH

is the name of a sentence, *viz.* of the sentence ' The moon has no atmosphere '. But will they ? And would they be right if they did ? (Is it not a curious kind of name which tells you of itself what it names—which, indeed, *embodies* what it names ?) And whether or not it is obvious that ' " The moon has no atmosphere " ' is the name (in English) of the sentence ' The moon has no atmosphere ', is it not perfectly obvious and certain that ' " The moon is round " is true ' (or ' " Queen Anne is dead " is true ', to return to our own examples) is *not* an ordinary English T-sentence ? I have not checked this assertion by statistical analysis of extensive observations in the field. But where a proposal shocks linguistic instinct, as I believe this one does, it is surely incumbent upon its advocates to produce the evidence that it is *not* inconsistent with ordinary usage ; and this Carnap does not appear to have done. (As he says, ' there are no factual assertions in pure semantics ' (p. 25), and it is in pure semantics that he is primarily interested. We, however, are interested in the bearing, if any, of pure semantics on ordinary T-sentences.) I am not, of course, maintaining that Carnap's alleged T-sentences are irregular in the way in which they would be if their grammatical subjects were *the R-sentences themselves* and not their names ; I do not accuse him of failing to realise that ' English grammar does not admit a sentence in the position of grammatical object ' (p. 52) or subject. I am merely maintaining that, as a matter of fact, it is not usual for English T-sentences to have as their grammatical subjects the names of sentences. Is it possible that Carnap has been misled into thinking that they sometimes do by supposing that the pronouns and descriptions which occur in such sentences as (B), (E) and (G) refer to or designate the sentence (A) ? But they do not ; they *replace* it, referring in an abbreviated or otherwise rhetorically suitable way to *what it refers to.*

The reason why names of sentences do not occur in ordinary English T-sentences (or perhaps it is another way of putting the same fact) is that people who utter ordinary English T-sentences are not thinking about the R-sentences. They are thinking about what the R-sentences mean—about what the speaker of the R-sentence is *saying.* (What he *says* is not the sentence he *utters :* how long would an interpreter keep his job who responded to the question ' What did he say ? ' by repeating the words of the speaker ?)

But if ordinary T-sentences do not contain names of their R-sentences, and if this is because (or perhaps another way of saying that) their speakers are not thinking about the R-sentences,

such T-sentences are not constructed in accordance with the Semantical Concept of truth, and such speakers do not apply it. Hence the Semantical Theory of truth will not, as Carnap claims, throw light on the ordinary concept of truth.   This is the main point I wish to establish.[1]

I do not wish to deny that it would be possible to talk and write in the way recommended by Carnap, although it is not at present usual to do so.   Suppose that somebody says that Queen Anne is dead and that what he says is true.   Then there is, as a mere matter of logic, a fact about the sentence he utters in saying that Queen Anne is dead, namely that this sentence— whether it was the sentence (A)—' Queen Anne is dead '—or the sentence (Z)—' Die Königin Anne ist tot '—has been used in making a true statement.   Someone might wish to state this fact ; and it would accordingly be convenient to introduce some conventions regarding the way in which he should state it. Whether it would prove conveniently self-explanatory or confusingly misleading to propose that facts of this sort should be stated in such sentences as ' (A) is true ' (or ' (Z) is true ', as the case might be), I do not know.   Perhaps the question would turn on the purposes in view and the number of parallel cases thought likely to occur.   But is there not a great danger that the construction of an artificial language of this sort would lead to the misunderstanding of the ordinary language it was supposed to parallel, which might not in fact be constructed on similar principles ?   May not the introduction of names for sentences, and of adjectives semantically correlated with properties of sentences, lead to the assumption that there are, in addition to sentences and facts, propositions, regarded as a third kind of entity, having the same sort of logical structure, properties and relations as sentences ?   And is not this metaphysics, in the bad sense ?   At any rate it seems clear that the value to philosophy of such semantical studies would be proportional to the care with which they were distinguished from the study of the ordinary ways of speaking in which philosophers are more directly interested.

If the foregoing argument is correct, Carnap's claim that semantics can elucidate the problem of truth is not well-founded. Let us see whether his Absolute Theory is more helpful.   But first let us pause to consider certain variants of the Semantical Theory which, though not expressly formulated by Carnap, are suggested by his theory.

[1] For a further objection to the Semantical Theory of truth see below, § 3 (b).

§3. *Crypto-Semantical Theories of Truth*

As we have seen, Carnap himself does not regard the Absolute Concept of truth as semantical, or even as semiotical. This need disturb nobody who is not committed to the view that truth is a semantical concept. Carnap's explanation of the use of the word 'true' in accordance with the Absolute Concept may well be valuable on its own merits. Nevertheless, the recognition of an absolute as well as of a semantical theory must surely strike everybody as detracting greatly from the interest, importance and plausibility of the claims which, as we saw, appear to be made by Carnap on behalf of semantical studies as an aid to the solution of philosophical problems. It seems worth while, therefore, to inquire whether there is any way of reconciling the existence of the Absolute Theory of truth with these claims. Are there, for instance, any considerations which would justify us in saying that the Absolute Concept of truth, although in one sense non-semantical, in another sense *is* semantical ? I think that among the possible interpretations of one or another of Carnap's dicta there are two which, though not (I think) intended by him, would support such an attempted reconciliation. These interpretations involve saying that truth in fact depends on the relations constitutive of signs, though it does not appear to do so. Truth, according to them, is a *crypto-semantical* concept. Shall we call them the ' Crypto-semantical ' Theories of Truth ?

(*a*) The less plausible of these Crypto-semantical Theories is suggested by Carnap's remark that (in accordance with the Absolute Concept) the term 'true' may be 'applied . . . to propositions as designata of sentences' (p. 26, quoted above). To understand this, we should require to understand the use of the word 'proposition', about which (as we shall see) Carnap tells us singularly little.

But for our present purpose a quite abstract consideration will suffice. We need only think of the proposition as something other than the sentence in which it is stated—something to which the sentence stands in the relation called by Carnap 'designation'. (We may *consider* the view that meaning is a two-term relation without accepting it.) We have, then, a pair of correlates, the sentence and the proposition, related, as sign and designatum, by the relation of designation. Now it can hardly be denied that if one of a pair of correlates has a certain predicate then there is a corresponding predicate which attaches, as a mere matter of logic, to the other : if my nephew has red hair then I am the uncle of a red-headed nephew. Answering to the absolute

concept of redheadedness applicable to my nephew there is an *avuncular* concept applicable to me, *viz.* the concept of the (relational) property of having a nephew with red hair. It is, I suppose, conceivable that occasion might arise for discourse about the relational properties of uncles ; and in that case existing language would probably be found deficient in concise terms with which to refer to them. A possible remedy—which in favourable circumstances *might* not be worse than the disease—would be to adopt the convention of using the existing names for the properties of nephews, with systematic ambiguity, for the corresponding properties of uncles. A little typographical ingenuity, assisted by plenty of suitable definitions, would suffice to make plain the distinction between the sense (literal) in which my nephew is red-headed and that (purely avuncular) in which I am say, Red-Headed. If now, we make the supposition that the set of avuncular concepts is in common use—so common that their relational character is often or usually overlooked—they might cease to be recognised as mere logical derivatives of the concepts of the ordinary or natural properties of nephews ; the term 'Red-Headed' and its fellows might be taken to indicate properties of uncles *simpliciter*. In such a case it would be a contribution to linguistic hygiene to point out that such terms were crypto-relational and specifically, since the relation in question is the avuncular relation, crypto-avuncular.

Something of this sort, we might suppose, is intended by Carnap when he introduces (§ 17, p. 88 ff.) a set of *absolute* alongside of the set of *semantical* concepts : true, false, implicate, equivalent, disjunct, exclusive. He might be saying that there is a set of concepts applicable to propositions in virtue of the relation in which they stand to sentences, *viz.* the relation of being the designatum of such-and-such a sentence. To every property which belongs naturally to sentences there will answer a (relational) property belonging to the propositions designated by such sentences. As the logical derivatives of the properties of the sentences, such properties of propositions—if their real nature is not observed—will be crypto-relational and specifically, since the relation in question is the relation of designatum to sentence, crypto-semantical. If the ordinary application of the word 'true' to propositions were a way of saying that the proposition in question is designated by a sentence which has in its own right the property of being true, it would be a contribution to philosophy to point this out and get it generally recognised that truth is, in this sense, a crypto-semantical concept.

Such an interpretation could, I believe, be placed upon the

expressions used by Carnap in introducing the absolute concepts corresponding to the semantical ones (§ 17). And it seems to be required if the recognition of the absolute concept of truth is to be reconciled with the view which Carnap seems to wish to advocate —that truth is semantical at bottom. But if this was his intention, ' absolute ' was surely a singularly unfortunate choice as the class-name for a set of concepts which (*ex hypothesi*) he thinks are mere logical derivatives of concepts properly applicable to signs. For it cannot but suggest the diametrically opposite view—surely much more likely at first sight to commend itself to common sense— that the use of ' true ' and similar words in application to sentences is a way of attributing to sentences (crypto-relational) properties which are the logical derivatives of properties of their *designata*, *i.e.* of propositions regarded as having independent existence. And whether or not Carnap wishes to suggest that the semantical properties of sentences are after all merely derivative, it does appear to be his view that the absolute terms indicate properties possessed by propositions independently : ' Since, however, the absolute concepts do not belong to semantics, their definitions need not take the roundabout way through semantics. . . . They can be stated in a straightforward and rather simple way. . . .' (p. 90).

The theory that truth is a property of sentences, but that answering to this there is a (crypto-semantical) property of propositions as designata of sentences, *viz.* that of being designated by some true sentence, would appear, then, not to be one that Carnap holds. Moreover, it is at first sight much less plausible than the contrary view, that truth is a property of propositions as independently existing entities, but that answering to this there is a (crypto-semantical) property of sentences, *viz.* that of designating some true proposition. It is not for that reason necessarily wrong. The greater plausibility of the latter theory is very largely due to assumptions about the meaning of the word ' proposition ' for which closer examination shows very little justification. Once the difficulties surrounding the use of the word ' proposition ' have been faced, the suggestion may well be made that ' proposition ' is itself a term definable only by reference to the (crypto-relational) properties which certain independently existing entities, or aspects of entities, acquire owing to the fact that someone uses a sentence to refer to them. And this suggestion, if it were made, might well carry with it a crypto-semantical theory of truth. But, since a consideration of its own merits as an account of the ordinary use of the word ' true ' would involve an elaborate discussion of those aspects

of the theory of judgment to which the use of the word 'proposition' is relevant, I propose to say no more about it here.

(b) There remains another train of thought which might lead us from some of the expressions used by Carnap in introducing the Absolute Concept of truth towards a crypto-semantical theory.

One of the differences, according to Carnap, between an absolute and a semantical concept is that the terms (or rather words) which we use for semantical concepts contain a reference to a semantical system (i.e. to an artificially constructed language) whereas those used for an absolute concept do not (p. 89). Thus we are instructed to say, using the semantical concept, '(A) is true in English', '(Z) is true in German'. This seems to me to show, as clearly as anything we have yet considered, how remote the semantical theory is from ordinary usage. If, in our ordinary T-sentences, we make no mention of their R-sentences, still less do we mention the language in which the R-sentence is uttered. And this is because, or is another way of saying, that we do not think that the language in which it is said makes any difference to the truth or falsity of what we say; truth and falsity, we may say, are linguistically neutral. It has often been pointed out [1] that a desire to account for the linguistic neutrality of truth and falsity is one of the principal sources of the belie   the existence of propositions as entities distinguishable   m sentences and from facts. A theory of truth asso   a belief in propositions so conceived is not, I take   sense a semantical or crypto-semantical theory. It might well be called an absolute theory, especially if it were contrasted with a theory which is openly semantical like Carnap's Semantical Theory. But, as Ayer has shown (loc. cit.), this is not the only possible way of saving the linguistic neutrality of truth. And reflexion upon the alternatives may lead us to a crypto-semantical theory.

Let us suppose that there are good reasons for holding that the word 'true' is applied, in the most fundamental sense, to sentences. Then a given T-sentence will be restricted to an R-sentence—(A), say—which is an individual and necessarily in some particular language. But from the fact that other sentences, some of them in other languages, mean the same as (A), it follows that any member of this class of sentences, which we may

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[1] E.g. by Ryle: *Are there Propositions?* Proceedings of the Aristotelian Society, 1929-30; Kaplan and Copilowish: *Must there be Propositions?* MIND, Vol. xlviii, No. 192, October, 1939; Ayer: *The Foundations of Empirical Knowledge* (London, Macmillan, 1940), p. 102.

call the ' neutrality-family of (A) ', is true if (A) is. It may some-
times happen—perhaps it happens more often than not—that
we wish to take account explicitly or implicitly of this fact about
the neutrality-family of (A). This, it may be thought, is done
by saying not that the sentence (A) is true but that a certain
proposition is true. This proposition will be thought of not as an
ersatz fact to which (A) refers but as an entirely genuine class or
family of sentences of which (A) is a member. Linguistic neutral-
ity will be preserved, because among the members of this class
or family are sentences belonging to different languages. The
class or family, of course, belongs to one no more than to another
of the languages to which its member sentences belong. The
fact that what is said about the proposition is after all something
said about sentences may be overlooked, and the concept of
truth applicable to propositions may seem not to be a semantical
concept. Anyone who wished to point out that nevertheless the
application of the word ' true ' to a proposition *is* a way of saying
something about sentences might think fit to do so by claiming
that truth is a crypto-semantical concept. To say this would be
to advocate a Crypto-semantical Theory of truth.

Granted the initial assumption that there are good reasons for
holding that ' true ' is ordinarily applied to sentences, this theory
seems to me far from negligible. On the contrary, it seems rather
to be a development the necessity of which it is surprising that
Carnap overlooked. It will clearly have to be carefully con-
sidered by anyone who wishes to hold a semantical theory of
truth, whether as an account of normal usage or as a proposal
to supplement or correct it. But it is not an absolute theory in
Carnap's sense ; and the initial assumption is one which I have
tried to show cause for rejecting. I shall therefore say no
more about it here, but shall pass on to discuss briefly Carnap's
Absolute Theory of truth.

§ 4. *The Absolute Theory*

Carnap's Absolute Theory of truth is the theory that the
word ' true ', in one sense at least, is applied to propositions. I
do not think that Carnap means us to understand by ' proposition '
what I have called above the ' neutrality family ' of a given
sentence. What exactly we are to understand by it is insuffici-
ently explained, either because Carnap did not, at the time he
was writing the *Introduction to Semantics,* realise the obscure and
controversial character of his assumptions or because he thought
of such discussions as insufficiently relevant to pure semantics.

From § 37, devoted to terminological remarks, we may indeed
learn that Carnap proposes to use the term not for sentences, but
for their designata, *i.e.* ' As " that which is expressed (signified,
formulated, represented, designated) by a (declarative) sentence "
(§§ 6 and 18). Other terms : " Satz an sich " (Bolzano), " Ob-
jectiv " (A. Meinong), " state of affairs " (Wittgenstein), " con-
dition " (p. 235). These terms are themselves, obviously, in
great need of clarification ; and a full discussion of what has been
or ought to be meant by the word ' proposition ' calls for a
separate inquiry. Pending such an investigation I can only
discuss the theory in a superficial way. I propose to inquire
briefly whether the Absolute Theory is consistent (*a*) with the
Identity Theory of truth and (*b*) with the ordinary use of the
word ' true '.

(*a*) I do not think that there is the same obvious kind of
inconsistency between the Absolute and the Identity Theories as
we found between the Semantical Theory and the requirement
of adequacy it was intended to satisfy. For there is not the
same definiteness about the Absolute as there is about the Seman-
tical Theory. Hence we do not seem, by saying that to assert that
a proposition is true means the same as to assert the proposition
itself, to commit ourselves, as the semantical theory does, to
making the T-sentence mention something other than what is
mentioned in the R-sentence. According to the Semantical
Theory, a T-sentence mentions something which the R-sentence
does not mention, *viz.* the R-sentence itself. But according to the
Absolute Theory we need not suppose that there is in the T-
sentence any mention of the R-sentence. If there is mention
of anything, presumably it is of the proposition which the R-
sentence asserts. But I do not think we know enough about
assertion and mention or designation to say that there is here any
objectionable difference between what is said in the T-sentence
and what is said in the R-sentence : all that the Absolute Theory
seems to require is that we should understand the utterance of a
T-sentence as a rhetorically different way of saying what would
be said by uttering the R-sentence—they are different ways of
asserting one and the same proposition. This is what is pre-
scribed in the Requirement of Adequacy, which I have called
the Identity Theory of Truth. It seems to me, then, that the
Absolute Theory does, while the Semantical Theory does not,
satisfy Carnap's own requirements as expressed in the Identity
Theory. In fact, it seems to me that the Absolute Theory is the
Identity Theory expressed in terms of propositions.

(*b*) Whether we feel that the Absolute Theory is or is not in

accordance with the ordinary use of the word ' true ' must, consequently, depend on whether we do or do not accept the Identity Theory of truth : *is* saying that it is true that Queen Anne is dead simply another way of saying that Queen Anne is dead ?

The obvious, and to my mind the principal, objection to this theory may be expressed in the form of a question : Why, if there is only one thing we want to say (*e.g.* that Queen Anne is dead) should there be different ways of saying it ? If the difference between a T-sentence and its R-sentence is not in the proposition asserted, why is there a difference at all ? Can we explain why there should be different ways of saying the same thing (over and above linguistic differences like that between German and French), and in particular what is the function of the difference between a T-sentence and its R-sentence ?

It is not difficult to think of an answer in rather general terms to the question why there should be different ways of saying the same thing. It is that language is a highly complex mode of behaviour performing distinct but not unrelated functions. Two speakers may use different sentences in making the same statement because they feel differently about the facts stated, or because they are talking to persons whose feelings about them are different, or because the respective audiences are differently equipped with knowledge relevant to the appreciation or even understanding of what is stated. But is this quite what is involved in the case of our T- and R-sentences ?

It does not seem to me that the difference between saying ' Queen Anne is dead ' and saying ' It's true that Queen Anne is dead ' is really very like the difference between saying (1) ' Queen Anne is dead ', (2) ' Alas ! Queen Anne is dead ', (3) ' Hurrah ! Queen Anne is dead '. If the difference between (A) and (H), for instance, is a difference in the attitude which they respectively express, it is at least not a difference of *emotional* attitude that is relevant. Suppose we were to call the difference between the emotion of sorrow (expressed by (2) above) and the emotion of joy (expressed by (3) above) a ' difference of quality '. Then I should be inclined to say that the difference between what is expressed by (A) and (H) respectively is not a difference of quality, but something more like a difference of *intensity*. Intensity of what ? Shall we say ' Of assertiveness ' ? Whatever W. E. Johnson may have thought about propositions, it is not necessary to agree with all of it in order to recognise that there is something in the direction in which he was pointing when he introduced the notion of differences of ' assertive attitude '. (*Logic*, vol. i, p. 3.) There is the difference between assertion

without doubting and reassertion in response to doubts—one's own or someone else's. (' Queen Anne is dead.' ' Do you seriously expect us to believe that ? ' ' It's *true*, I tell you ! ') That there are differences in the firmness of the conviction we wish to express, or in the intensity of the effort needed to overcome the recalcitrance of others in face of our attempts to evoke belief, may be one reason for using sometimes a T-sentence instead of an R-sentence.

Another sort of reason has been indicated by Ayer (*Foundations of Empirical Knowledge*, pp. 102, 103). In some cases a T-sentence does not so much *repeat* its R-sentence with added (' It's *true*, I tell you ! ') or diminished (' Well, yes, that's true, but . . .')—at any rate with altered—emphasis, as *abbreviate* a collection (' Everything you have said is true ') or a described class (' Whatever he says is always true ') of sentences. Even here, however, the function of the word ' true ' seems to be to express assertive attitude—agreement, or insistence, as against the opposition or hesitation expressed by ' That's false ' or ' That's doubtful '.

We need not labour the question whether these are two accounts of the use of T-sentences, or two varieties of the same account, applicable to different varieties of situation in which T-sentences, rather than any others, are what we ordinarily use. The main point is that it has been easy to show why there should be different ways of saying the same thing, even where differences of emotional attitude to what is said are left out of account. So far as can be seen without a much fuller discussion of the use of the word ' proposition '—for which this is not the occasion—there is no need to seek for the explanation of the meaning of the word ' true ', in its ordinary use, by looking for a quality or relational property of propositions regarded as *ersatz* facts or quasi-things. We can, then, agree that the Absolute Theory (= for present purposes the Identity Theory) *is* in accordance with the ordinary use of the word ' true '.

§ 5. *What Kind of Concept* ?

If the foregoing arguments are correct, truth is not a semantical concept, because T-sentences are not about their R-sentences and do not make or involve any assertion to the effect that the R-sentences designate or mean this or that. Is Carnap right in holding that truth, in this use of the word ' true ', is not a semiotical concept at all ? It seems to me that he is not ; and though the point is clearly not of great importance it may be worth while to consider it briefly.

Carnap's view that what he calls the 'absolute' concept of truth is not in any way semiotical reflects, I suspect, a certain inadequacy in his analysis of the functions of sentences, and in particular his failure to examine closely the meaning of the word ' assertion '. As we have seen, he regards the use of the word ' true ' as intimately connected with assertion : ' to assert that a sentence is true means the same as to assert the sentence itself ; *e.g.* the two statements " The sentence ' The moon is round ' is true " and " The moon is round " are merely two different formulations of the same assertion ' (p. 26). But of how he wishes us to understand the words ' assert ' and ' assertion ' he tells us nothing at all. His use of the phrase 'to assert the sentence itself ' suggests, as I hinted above, that he takes the word ' assert ' to be used in much the same way as the word ' utter ', as equivalent perhaps to ' speak or write '. Hence he does not look for the meaning of the words ' assert ' and ' assertion ' in the *function* of the (spoken or written) sentence. As is perhaps justified in a purely semantical study, the only function of sentences with which Carnap concerns himself is the one which he calls ' designation '. This is the supposed relation of a sentence to a proposition, and is conceived by Carnap on the analogy of the relation of the single word to what it means. Very roughly, we might say he supposes that there are a lot of propositions and that the semantical function of the sentence is to direct attention to this one rather than that.

Without entering further into speculations about Carnap's state of mind, we can perhaps suggest that a more ordinary use of the words ' assert ' and ' assertion ' would be exemplified in the statement that the function of a (declarative) sentence is not merely to indicate one proposition rather than another but to *make some assertion,* to *assert* the proposition indicated. And what do we do when we make an assertion, *i.e.* assert a proposition ? Do we not express our own belief in it, and/or seek to induce in others a belief in it ? If you like, we express and/or seek to induce a favourable assertive attitude towards it, whereas in saying that it is false that . . . we should be expressing and/or seeking to induce a negative or unfavourable attitude. (But if we adopt this formula we must be careful not to suppose that a proposition is a quasi-thing which we look at smilingly or frowningly.)

Assertion, then, is not something that is done to a sentence. Still less is it something which occurs among things or facts irrespective of what is thought or said about them. It is one of the functions of (declarative) sentences, distinguishable from their

function of singling out propositions. And if, as we have seen to be plausible, the function of the word ' true ' is the function of assertion, then truth, though not a semantical, *is* a semiotical concept. Specifically, it is a concept involving that function of language concerned with our activity in expressing ourselves and seeking to influence other people. It is therefore to be called, if we follow Morris, a *pragmatical* concept. That the states with the production of which it is specially concerned are beliefs rather than emotions or actions makes no difference to this. What are sometimes called ' cognitive states ' do, of course, differ in certain respects from actions and emotions. But they resemble them in being sometimes expressed or evoked by the utterance of sentences.

The recognition of the function of the word ' true ' as pragmatic, *i.e.* as related to the function of symbols in expressing and/or inducing states of mind, would have the advantage of explaining why it is often quite natural to use ' true ' in contexts where the state of mind to be expressed or induced would not be called by everyone a cognitive state, *e.g.* in normative contexts. ' Is it true that I ought to love my enemies ? ' ' It's true that the Albert Memorial is a monstrosity.' ' Meet you at the Tate at three.—All right.—But will you be there at three ?—Yes, really and truly I will, *this* time.' Such sentences can be brought under an extended version of the Identity Theory of truth. For we may lay it down that a person who utters a T-sentence is or should be using it to say, with only such differences as abbreviation, generalisation, intensification or mitigation, whatever would be said by uttering the R-sentence or any equivalent sentence. Since sentences are by no means confined to the semantical function, it is clear that such a rule for the use of the word 'true' would employ a concept which, though semiotical, is not semantical in a narrow sense. If it turns out (as I think likely) that the function of expressing and/or inducing states of mind is always present, and is the function to which the use of the word ' true ' is more particularly relevant, then the concept of truth may, as I have already suggested, be called *pragmatical*.

II.—THE IMMORTALITY OF THE SOUL

By A. H. Basson

S. Greetings Crito, we only await your arrival.

C. Forgive me Socrates. I was called suddenly to the house of Heraclitus, who is seized with the pain we call gout.

S. I am truly sorry to hear that. But tell me Crito, how do you know that poor Heraclitus is seized with this pain ? Does he say so ?

C. No Socrates, he does not say that. His manner of speaking is obscure, as you well know.

S. How then did you diagnose his illness ?

C. That is easy. The afflicted limb is swollen by an influx of fiery humour. The manner of the patient is the manner of one who suffers pain in this limb, and this pain, which is caused by an aggregation of the fiery humour, is the pain we call gout.

S. Tell me this Crito, can you relieve this pain which you call gout ?

C. Yes indeed, an infusion of poppy seeds will relieve any pain.

S. But it will not dissipate the fiery humour ?

C. No it will not.

S. Do your patients sometimes suffer pain, even when no fiery humour is present ?

C. They do.

S. So that from the presence or absence of the fiery humour alone, you cannot tell whether pain is present or absent ?

C. That is so.

S. How then do you decide ?

C. Why, it is from the manner of the patient, Socrates, and whether he complains of pain.

S. But a man of fortitude may suffer and yet not betray it ; and a play actor may counterfeit the manner of one who suffers, and yet feel nothing. Do you not agree to this Crito ?

C. I know that very well. But there are certain signs which cannot be disguised, even by the strongest, and which cannot be counterfeited, even by the most ingenious. It lies in the art of the physician to distinguish such signs.

S. Are these signs which you describe the same with the suffering, or distinct from it ?

C. Truly they are distinct, for the signs which I distinguish are

23

agitations of the body, whereas suffering is an agitation of the soul. But these signs are always attended by suffering, and suffering is always attended by these signs.

S. Would you say that all the agitations of the body are of this nature ?

C. I do not think so. Some are under the control of the patient, and we cannot judge by such how he truly feels.

S. Otherwise fortitude and play acting would alike be impossible ?

C. Just so.

S. Then the first task of the p¹ ˥sician is to distinguish those signs whereby he can truly judge how his patient feels, from the others ?

C. That is what I have said.

S. Now tell me this Crito, have you any way, other than the way of bodily signs, for judging truly how your patient feels, and whether he suffers pain or no ?

C. Certainly not Socrates. For if we had another way, it would not be necessary for a physician to learn to read the signs.

S. How then do you discover which bodily signs are always accompanied by suffering in the way you describe ?

C. I . . . This is a hard question Socrates. I can only say that these things are known to every physician, and that he learns them from his masters and from experience.

S. And his masters, how do they learn ?

C. Why, from their masters, I suppose ; or from experience ; or perhaps the knowledge is implanted in them from the beginning.

S. So that in the first instance, either the knowledge must be gained from experience, or it must be implanted from the beginning ?

C. Quite so.

S. Now tell me this Crito, could the knowledge of which we speak have been discovered in the first instance by experience ?

C. Many such things are so discovered, Socrates. For example, we know by experience that the swelling and inflammation of an afflicted limb is caused by an aggregation of fiery humour within. This was discovered by the ancient physicians, who made an incision in the limb of a patient who had died, and so observed the fiery humour. The observation has been repeated by myself, amongst others, and whenever swelling and inflammation have been present, and an incision has been made, the fiery humour has been discovered. Thus we know that these signs, the swelling and inflammation, always signify the presence of a fiery humour.

S. And do you make this incision in every case, Crito ?

C. No Socrates, we do not. It is supposed that men are alike in this respect. And it seems to me that this supposition is quite fair and reasonable, and amounts to knowledge.

S. You would describe this by saying that the fiery humour causes the afflicted limb to swell and become inflamed, would you not ?

C. That is what I should say.

S. Would you also say that the agitated manner of a patient is caused by the pain he feels ?

C. Certainly.

S. So that these two cases are exactly alike ?

C. Yes.

S. But you cannot look into a man's soul and observe his suffering, as you can look into his body and observe the fiery humour. The agitations of the soul are invisible to us, are they not ?

C. Quite so.

S. So that the two cases differ in this respect ?

C. That is true, but I do not think the difference is an essential one. I should still say that a pain causes a certain agitation in the manner of the patient.

S. Just now you described to me very clearly, Crito, how a cause which is hidden may be revealed. You described how, in cases of swelling and inflammation, an incision has been made, and a fiery humour revealed. Upon this you conclude in like cases of swelling and inflammation, where no operation has been performed, that if the operation were to be performed on this patient, the fiery humour would be revealed.

C. You have my meaning, Socrates.

S. Now, Crito, upon what evidence do you conclude that a feeling of pain causes a certain bodily agitation, since the former cannot be revealed to us by any operation ?

C. Why, the two cases are somewhat similar, Socrates. The agitations of our own souls are revealed to us, and we observe that certain agitations of our body are always attended by certain agitations of our soul. Since other men are like us in other respects, we conclude that they are like us in this respect.

S. What does your conclusion amount to, Crito ?

C. It amounts to this : that if a man presents a certain agitation of manner, then I know he suffers pain. I know that if the workings of his soul were revealed to me, as the workings of my soul are revealed to me, then his pain would be revealed to me, as my pain is revealed to me.

S. Tell me, Crito, do you perform any operation in order to view the workings of your soul ?

C. In some cases perhaps.   But it lies in the nature of a pain that, if it exists, it must be revealed to the sufferer, without any special observation on his part.

S. Let us suppose, Crito, that you had observed that a certain agitation of your body had always been attended by a feeling of pain on past occasions ;   and one day you experienced this agitation, without experiencing the usual accompanying pain. Would you conclude that on this last occasion you suffered a pain which you did not feel, or would you conclude rather that on this occasion no pain was present ?

C. I should conclude that no pain was present.

S. Suppose you found that the performance of a certain operation was always followed by a feeling of pain, or sometimes followed by such a feeling and sometimes not.   What would your conclusion be ?

C. I should conclude that the operation caused the pain, or was part of the cause, or there was no connexion.

S. You would not conclude that the operation revealed the pain to you ?

C. By no means.

S. It lies in the nature of a pain, does it not, that it cannot be said to be hidden, and so it cannot be said to be revealed by any operation either upon the body or upon the soul ?

C. That is so.

S. Now tell me Crito, when your patient suffers pain, would you say that his pain is hidden from you ?

C. Certainly.

S. By what operation, then, is it revealed to you ?

C. I cannot answer that, Socrates.   I do not know of any such operation.   But this does not mean that the operation does not exist, or is not possible, but only that I am ignorant of it.

S. Of the operations which you can perform, some are attended by a feeling of pain on your part, and the others are not so attended.   Is this not so, Crito ?

C. Truly Socrates, all operations must satisfy one of these conditions.

S. Of the operations which are not attended by a feeling of pain on your part, would you say that any of these revealed to you another man's pain ?

C. Certainly not.

S. Of the operations which are attended by a feeling of pain on

your part, would you say that these revealed pain to you, or that they caused you pain ?

C. I should say they caused me pain.

S. Now consider, Crito, does the pain so caused exist before the performance of the operation, or not ?

C. It does not.

S. And your patient's pain, does that exist before the operation, or not ?

C. Yes, it does.

S. Is then the pain you feel on performing the operation the same with the pain felt by your patient, or different from it ?

C. Clearly they are different, Socrates.

S. Can you then say of any such operation, that it reveals to you the pain suffered by your patient ?

C. It seems I cannot.

S. It follows then, does it not, that no operation whatever can be said to reveal to you the agitations in the soul of your patient ?

C. I cannot escape that conclusion.

S. Now tell me Crito, do you say a thing is hidden, if no operation whatever can be said to reveal it to you ?

C. Yes, I think we may say that, Socrates. I should say that such a thing is hidden by nature, or it is by nature invisible to us.

S. And do you consider the soul to be such a thing ?

C. Assuredly I do, Socrates. I conceive it after this manner. Suppose there existed a race of men, such that each man was visible only to himself. Suppose no action on the part of any such man could reveal any other man to him. Then I say the bodies of these men are invisible by nature, just as the souls of all men are invisible by nature.

S. How then does any such man know that others of his kind exist ?

C. That is easy. If such a man sees a hoe moving in a field, he knows that another man wields it, for hoes do not move of themselves. If he sees some garments assuming the form of a man, he knows that they clothe another man, for garments do not assume this form of themselves.

S. Do you then conceive the body as it were the tool or vestment of the soul ?

C. I do.

S. Can we not conceive of some other Power wielding the hoe, or causing the garments to assume this form ?

C. That is possible.

S. And the form and motions of this Power would not be those of a man ?

C. Perhaps.

S. So that from the form and motions of the hoe and of the garments alone, we cannot discover the form and motions of the invisible Power that moves them. We cannot truly decide whether this is a man or some other Power ?

C. I must agree.

S. So if your simile is correct, we cannot truly know the form and motions of the Power which moves the body of a man, whether this is the soul of a man or some other power ?

C. That must be so, if my simile is correct.

S. Do you accept this conclusion, Crito ?

C. No, Socrates, I cannot accept it.

S. Then your simile must be incorrect in some particular ?

C. I am forced to that conclusion, Socrates. Perhaps you can show me in what particular my simile is incorrect.

S. Willingly, my dear Crito. Consider, what would enable us to decide whether the Power which moves a hoe is a man or some other power ?

C. Why, if the man were visible to us, Socrates, then we should know.

S. Our ignorance, then, is the result of a defect in our power of seeing. We say that if we could see, then we should know ?

C. Certainly.

S. The men in your fable, Crito, they also could say this, could they not. They could say : If we could see one another, then we should know what powers move the things about us ?

C. Just so.

S. The ignorance of these men lies in the fact that certain experiences are denied to them ?

C. Yes.

S. But our ignorance of the souls of other men lies, not in this fact, but in the fact that no experience whatever can be said to reveal it ?

C. That is true.

S. We cannot then be said to be ignorant of the souls of other men, just as the men in your fable are said to be ignorant of the bodies of other men ?

C. I must admit that difference, Socrates. Our ignorance of the souls of other men is not the same with the ignorance of the men in my fable.

S. Then what is the nature of our ignorance ?

C. It lies in this, Socrates. That we cannot know the thoughts and feelings of other men, as we know our own thoughts and feelings. The souls of other men are hidden from us, and you

have convinced me that they are hidden in a peculiar manner, so that they cannot be revealed to us by any means. The gods themselves cannot reveal these things to us.

S. Tell me, Crito, do you conclude from this that you cannot know what another man thinks, nor how he feels ?

C. No, Socrates, I cannot accept that conclusion.

S. Do you then conclude that, when you say another man suffers pain or enjoys pleasure, you mean only that he behaves in a certain way, namely, the way you yourself behave when you suffer pain or enjoy pleasure ?

C. Certainly not.

S. Then what is your conclusion ?

C. I am at a loss what to conclude, Socrates. For you have put me in a position where it seems that I must draw one conclusion or the other, and yet I know that both of these are false. Sometimes I know how another man feels, and I know that his feeling is something other than the bodily behaviour which I observe. Nevertheless, my judgment is founded solely upon what I observe.

S. Consider this, Crito. When we say a man hears a sound, we do not simply mean that he behaves in a certain way ?

C. Certainly not. We mean also that there is a sound to be heard.

S. Suppose a man behaves as one who hears a sound, when there is no sound to be heard ?

C. Then we say he is play acting, or that he suffers an hallucination.

S. What is an hallucination ?

C. It consists in hearing as it were a sound, when there is no sound to be heard.

S. Suppose on some occasion a man behaves as one who hears a sound, and there is indeed a sound to be heard, but afterwards he assures us that he was play acting and that he heard no sound. What would your conclusion be ?

C. I should conclude that his memory deceives him, or that he is lying on the second occasion.

S. Would you conclude that perhaps he did not hear the sound ?

C. No I should not.

S. In this case, then, we know better than the man himself how he felt on the past occasion ?

C. Yes.

S. But on the present occasion, the man knows better than we can ever know, how he feels ?

C. Just so.

S. But our future knowledge may be better than his future knowledge, since his memory is not altogether reliable ?

C. Yes.

S. So his future knowledge must likewise be inferior to his present knowledge ?

C. Assuredly.

S. Now tell me, Crito, what is it that he now knows better than he will ever know in the future, and better than we shall ever know. What is the nature of the judgment that he makes ?

C. He knows that he feels a certain kind of feeling, Socrates.

S. He knows that his feeling resembles other feelings he has felt, and which other people have felt, and differs from yet others ?

C. That is so.

S. But some of these other feelings are better known to us than to him, are they not ?

C. Yes.

S. So that he cannot know better than we do, whether or not his present feeling resembles these other feelings. His superior knowledge does not consist in this ?

C. No, it seems not.

S. Then what is the nature of his knowledge ?

C. He knows better than we do, whether or not he is play acting.

S. What is it for a man to be play acting, Crito ?

C. When his sentiments do not justify his actions, that is play acting. A man may choose his actions, but not his sentiments.

S. Do you agree to this, Crito. When a man suffers pain, he knows the character of his feeling, and this consists partly in the knowledge that what he now suffers, resembles what he has felt in the past and what other people feel ; but in this respect his knowledge is not better than ours. Another part of his knowledge consists in this : he knows that the feeling he now has impels him to take certain action, but he may or may not choose to take this action. Hence in this latter sense, he knows better than we ever can how he now feels. For if he does not choose to take the action to which he is impelled, we shall be misled.

C. Quite so.

S. Suppose a man exhibits all the signs of one who suffers toothache, and on investigation you find an abscess. Would you say he may be play acting ?

C. Certainly not.

S. Suppose he says that what he feels is not toothache ?

C. I should assure him that it was.

S. You would know better than he ?

C. Certainly.

S. In these circumstances it is impossible that a man should not suffer toothache ?

C. Just so.

S. Now tell me, Crito. Is it not possible for you to imagine yourself in this situation. Can you not imagine yourself having an abscess and exhibiting all the signs of toothache, and yet not feeling any pain ?

C. That is a different point, Socrates. Certainly I can imagine myself inclined to say that I suffered no pain. But on a broader view I should say that this statement could not be true. For there must be some feeling which leads to my behaviour in these circumstances, and my judgment that it is not a pain, would arise from the fact that it did not seem to me that this feeling resembled the feeling I had felt on similar occasions in the past. On reflexion I think I should conclude that my memory deceived me, since the present feeling must be a pain.

S. But suppose no feeling whatever moves you to exhibit the bodily signs you do in fact exhibit ?

C. That is not possible, Socrates.

S. Can you not imagine the situation I describe, Crito. A human body exhibits a certain condition, and yet you yourself feel nothing ?

C. Certainly I can imagine that. It is a common enough experience. But I describe this by saying that the body in question is not my body.

S. In these circumstances, you do not conclude that no soul invests this body, but only that your soul does not ?

C. Just so.

S. How, then, do you decide whether a soul invests this body, or not ?

C. By the manner in which it moves, Socrates.

S. When a human body moves after the manner of men, you say that it is invested with a human soul ?

C. Certainly.

S. And when it is not moved by you, it is moved by another ?

C. Yes.

S. So that when we say there are other souls, we do not merely mean that bodies move, but also that these movements are not commanded by us ?

C. Agreed. For if the motions were commanded by us, they would not provide evidence for the existence of other souls.

S. Now does the absence of these motions in a body prove the absence of a soul ?

C. By no means. A man may lie as one dead, and yet still be aware of all that happens about him.

S. How can we know that, Crito ?

C. When he recovers he may tell us. He will remember his experiences.

S. Suppose he does not recover ?

C. Then we cannot tell.

S. So that when a man dies, we cannot tell whether his soul remains in place, or departs, or is altogether destroyed ?

C. I cannot venture an answer to that question, Socrates. I think it may well be disputed throughout all subsequent ages, and no conclusion gained.

S. You mistake my meaning, Crito. I do not ask you to say what happens to the soul when the body dies, but whether it is possible to conceive of an answer to this question ?

C. I think it is possible to conceive of an answer. The souls of the dead might return to earth in another form. For example, if Plato died, and a child were born, who grew in the course of time to the vigour and intellectual power of Plato, we should say : Here is a second Plato. But if he grew also to have the memories of Plato, and spoke to us of the old times, we should say : Here is Plato born again.

S. Does this in fact happen, Crito ?

C. No it does not.

S. Must we conclude then, that the soul dies with the body ?

C. Surely the soul may survive, and not be reborn ?

S. I ask you, Crito, what could it possibly mean to entertain such an idea ?

C. It is hard to say, Socrates, but I think it may be put in this way. If the events I have just described were the common thing we should of course come to expect them, but we should expect another thing as well. We should expect, and with good reason, that when we ourselves came to die, we should afterwards find ourselves in other circumstances. The thought of death would lose its power over us. But even though the events I described do not happen, it does not follow that after death we shall not find ourselves in other circumstances.

S. You say that, if certain things occurred, these would serve to raise our hopes of immortality ; but nothing that occurs can altogether destroy these hopes. Does it not follow that the soul must be immortal, and the only question can be the manner of its immortality ?

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C. I do not see why. Our hopes may be indestructible, but it does not follow that they are justified. For they are indestructible only in this sense : that only if we are destroyed are they destroyed.

S. The couch upon which I now recline may be utterly destroyed by fire. I employ a cabinet-maker to build me a new one, exactly resembling the old. Is this new couch the same as the old one, or different from it ?

C. Truly it is different, Socrates, for that which is altogether destroyed cannot be recreated.

S. If a thing is truly said to be destroyed, we cannot conceive of its existence at a later time, although we can conceive of the existence of something exactly resembling it ?

C. Just so.

S. Are there any circumstances when we can truly say of a thing, that we cannot conceive of anything exactly resembling it ?

C. Certainly not.

S. So that if we could truly say that a thing is destroyed, only if we could not conceive either of its existence at a later time, or of the existence of any thing exactly resembling it ; we could never truly say that this thing is destroyed ?

C. Quite so.

S. Now suppose the body of Plato is destroyed, and another human body exhibits the powers and habits and memories of Plato, and speaks after the manner of Plato. You can conceive of this, can you Crito ?

C. Certainly I can conceive of it.

S. Would you then say that this body was invested with a soul exactly resembling the soul of Plato ?

C. I should say it was the soul of Plato.

S. So that to say that the soul in question exactly resembles the soul of Plato, and to say it is the soul of Plato, is to say the same thing ?

C. Yes.

S. It follows, does it not, that in order to speak truly of the destruction of Plato's soul, it must be impossible for you to conceive of the existence of anything exactly resembling it ?

C. That must be our conclusion, Socrates.

S. But under these circumstances, we can never truly say of the object in question that it is destroyed ?

C. Yes, I have agreed to that.

S. I conclude, therefore, that Plato's soul can never truly be said to be destroyed. I conclude that the soul of Plato is immortal.

C. I can detect no flaw in your argument, Socrates, and yet I am not altogether convinced.

S. Tell me Crito, does your dissatisfaction spring from the character of the argument, or from the nature of the conclusion ?

C. From the nature of the conclusion, I believe. Your proof of immortality serves equally well to establish the continued existence of the soul when a man is asleep or unconscious from a blow. I do not dispute the correctness of your proof. Death may be no more than a long sleep, a sleep from which there is no awakening in this world, as we have every reason to believe. But we have no evidence at all, one way or the other, to tell us whether or not there is an awakening in another world, or whether or not dreams come to us in the sleep of death.

S. Tell me, Crito, what is the nature of the evidence which would enable us to answer these questions ?

C. Alas, Socrates, I cannot say.

III.—SENSE-DATA AND THE PERCEPT THEORY

By Roderick Firth

Part Two

THE EPISTEMOLOGICAL IMPLICATIONS OF THE PERCEPT THEORY.

The most revolutionary inferences which philosophers have drawn from the Percept Theory are probably those which concern the epistemological status of physical objects ; we have already seen, for example, that in recent years some philosophers have used the Percept Theory as a basis for attributing to physical objects the same epistemological status that has traditionally been attributed to sense-data.   But there is another possible implication of the Percept Theory which deserves prior consideration because our decision concerning its validity will necessarily influence our analysis of almost all other epistemological issues.

1. The Given and its Interpretation.

We might wonder, specifically, whether acceptance of the Percept Theory can force us to deny completely a fact the recognition of which Lewis has called " one of the oldest and most universal of philosophic insights ", the fact, namely, that " there are, in our cognitive experience, two elements : the immediate data, such as those of sense, which are presented or given to the mind, and a form, construction, or interpretation, which represents the activity of thought ".[1]   The distinction to which Lewis refers is, in one form or another, so fundamental to most philosophical and psychological systems, that to reject it entirely would necessitate, at the very least, a complete reformulation of these systems.   And despite the phenomenological evidence for the Percept Theory, there are undoubtedly many philosophers and psychologists who would find that theory quite incredible if it could be shown to imply that the distinction between what is given, and the interpretation or construction put upon it, is entirely invalid.

[1] *Mind and the World-Order*, p. 38.

But there are, I believe, three rather different senses in which this familiar distinction can be recognised even by those who accept the Percept Theory; and there is only one traditional sense in which the distinction must be denied.

(a) *The Given as the Ostensible Physical Object.*

In the first place it must not be forgotten that the Percept Theory is a theory about perceptual *consciousness* and that the evidence for it is entirely phenomenological and gathered by direct inspection of many single states of perception. If, therefore, the terms " construction " and " interpretation " are defined *dispositionally*—by reference either to a tendency towards bodily behaviour of a certain kind or to a tendency to have certain kinds of conscious experience under certain conditions, or both— then the validity of the distinction between " the given " and its " interpretation " is entirely independent of, and hence compatible with, the truth of the Percept Theory. Thus for those who accept the Percept Theory the things that are given in perception would be ostensible physical objects, these being the only sensuous constituents of ordinary perceptual consciousness; and the manner in which these are interpreted would be determined by discovering the dispositions which accompany them. In many contexts, moreover (*e.g.*, in most discussions of learning) the philosophers and psychologists who have distinguished between the given and its interpretation have intended to say nothing that is incompatible with such a theory of the given; to recognise a distinction of this kind, therefore, is to admit the validity, in one historic and important sense, of what Lewis has called " one of the oldest and most universal of philosophic insights ".

But that is not all that can be admitted, for it should be remembered that the Percept Theory, as I have described it, is a theory limited not only to perceptual consciousness but to the *sensuous* aspects of perceptual consciousness. It is a theory, to be more precise, about the phenomenological status of the ostensible physical object and the sensuous qualities which clothe it, and it is incompatible with the Sense-datum Theory, as we have seen, precisely because it denies the phenomenological duality of any of these sensuous qualities. If, therefore, a philosopher or psychologist happens to believe that the ostensible physical object usually does not *exhaust* the content of the consciousness during perception; if, for example, he believes that the ostensible physical object is presented together with certain

bodily feelings, or with an " ostensible self " or with an " ostensible perceiving self ", or with phenomenologically irreducible " beliefs " or " judgements " or " expectations ", or indeed with any other possible constituents of consciousness whatsoever ; and if, at the same time, he does not believe that any of these constituents are sense-data as traditionally conceived, and is therefore able consistently to admit that sensuous qualities are presented as the qualities of the ostensible physical object and not of any other entity ; such a philosopher or psychologist, so far as I can see, does not believe anything that is incompatible with the Percept Theory. It might consequently be possible, by defining " interpretation " and " construction " in terms of some of these other constituents of perceptual consciousness, to give a purely phenomenological meaning to these words so that the given (the ostensible physical object) could be distinguished from its interpretation by direct inspection of perceptual consciousness. Thus the historical distinction could be recognised in another sense, and a sense which would probably represent the principal point that Lewis himself has in mind when he says : " That present datum of experience which is interpreted as ' activity of thought ' is just as objective and intrinsically observable a kind of datum as is the phenomenal appearance of an external object ".[1] Whether or not direct inspection of single states of perceptual consciousness *can* validate such a distinction is a difficult question, and one which is not strictly relevant to the basic issue under discussion ; but there is nothing in the Percept Theory to imply that it cannot.

Some of the examples which Lewis gives to illustrate the distinction between the given and its interpretation, however, require that this distinction be recognised in another sense, quite different from the two so far discussed ; and in this other sense the distinction is not compatible with the Percept Theory. Lewis points out the well-known fact that our perceptual experience varies not only with changes in the physical conditions of observation but also with changes in interest, and he illustrates this by showing that the perceived qualities of a fountain pen differ for a child, a writer, and a savage. This *fact*, of course, is quite compatible with the Percept Theory, for the characteristics of ostensible physical objects do indeed vary with the attitude of the perceiver. But Lewis uses this fact to illustrate the difference between the given and its interpretation, and the distinction in this instance is drawn in a manner which is incompatible with the Percept Theory.

[1] *Mind and the World-Order*, p. 424.

The distinction is drawn between a " presentation ", which is supposed to be the constant and given element in the various perceptual experiences of the pen, and its " meaning " or interpretation. Speaking, for example, of the fountain pen in his hand, Lewis says : " It might happen that I remember my first experience of such a thing. If so I should find that this sort of presentation did not then mean ' fountain pen ' to me." [1] But since by the expression " this sort of presentation " Lewis means a complex of qualia or sense-data [2] which are to be distinguished from the qualities of the ostensible pen, it is clear that this expression, for those who accept the Percept Theory, simply has no designatum at all within these states of perceptual consciousness. According to the Percept Theory there simply is no common core of sense-data to " mean " one thing at one time and another thing at another time. It is possible, to be sure, that the ostensible physical object presented in childhood might have had certain properties (*e.g.*, a particular shape and colour) in common with the ostensible physical object presented at the time of writing ; in fact this is probably what it would ordinarily mean to say that the two presentations were " of the same sort " ; but the particular distinction which Lewis has in mind, and which is essential to the Sense-datum Theory, is not one that can be defined by reference solely to the properties of the ostensible physical objects.

The problem, therefore, for those who accept the Percept Theory, is whether in rejecting this distinction between the given and its interpretation they must also reject as meaningless all the epistemological and psychological principles whose formulation presupposes that the distinction is valid. In view of the historical importance of the Sense-datum Theory it is clear that this problem cannot be lightly dismissed. Whatever one may think, for example, of the ultimate value of the introspective psychology of Wundt and Titchener, it will stretch the credulity of those who are familiar with their experimental work to suggest that the principles which they formulated concerning the relationship between sensation and " meanings " are completely *meaningless*.[3] The conception of the sensory core, moreover, appears to have a certain methodological value, for differences and similarities among sensory cores have been supposed to provide psychologists with phenomenal criteria for deciding just what characteristics of perceptual consciousness can and cannot

[1] *Mind and the World-Order*, p. 49.   [2] *Ibid.*, p. 60.
[3] *Vide* Titchener, *A Beginner's Psychology*, ch. 1.

be explained by reference to physical processes in the sense-organs ; it has been supposed, for example, that the fact that two perceptions have similar sensory cores guarantees that all differences between these perceptions must be explained by reference to attitudes (broadly interpreted) and the physiological conditions of attitudes. And it might not be easy, even for those psychologists who accept the Percept Theory, to dispense entirely with such methodological principles.

(b) The Given as the Product of Perceptual Reduction.

It seems to me, however, that the solution to this problem is not so difficult as it may appear, for I believe that we can find for these particular psychological purposes, a completely satisfactory substitute for the sensory core as traditionally conceived. We can do this by applying the pragmatic maxim and asking ourselves just how psychologists have actually *decided* whether or not two perceptions are to be called " interpretations of the same sensory core ". And if our methodological analysis in the previous section is correct, this has been decided, of course, by subjecting the two perceptions to the operation of perceptual reduction and comparing the resulting states of direct awareness. If, to use Lewis's example, two different perceptual experiences of a fountain pen are perceptually reducible to direct awareness of similar sense-data—perhaps long tapering patches of black—then it would be concluded, no matter how different the ostensible physical objects, that the two per-ceptions are " different interpretations of the same given ". For those who accept the Percept Theory, therefore, this " method of verification " can be used to define the term " sensory core " in a way which will provide a substitute for the traditional concept. We can say that the statement " These two per-ceptions are different interpretations of the same sensory core ", should be understood to mean : " If these two perceptions were perceptually reduced exactly similar states of direct awareness would be produced in the two cases ". And to understand this second statement, of course, we do not need any concepts which are incompatible with the Percept Theory.

Ayer has called such pragmatic definitions as this " definitions in use ". " We define a symbol *in use* ", he says, " not by saying that it is synonymous with some other symbol, but by showing how the sentences in which it significantly occurs can be translated into equivalent sentences which contain neither the *definiendum*

itself, nor any of its synonyms ".[1] By means of this definition
in use, then, philosophers and psychologists who accept the Percept
Theory can translate into an empirical language statements about
the given which would otherwise be verifiable only if the Exposure
Hypothesis were valid. In preferring this definition, moreover,
they do not necessarily belittle the importance for psychology of
either the operation of perceptual reduction or the concept of the
sensory core which is defined in terms of it. To deny the exis-
tence of the sensory core as traditionally conceived, therefore, is
not necessarily to discredit the empirical science erected by
psychologists who have assumed its existence, nor even to dis-
parage their method.

Thanks to the definition in use, therefore, there is a third
sense in which those who accept the Percept Theory may re-
cognise what Lewis has called " one of the oldest and most
universal of philosophic insights ". And once a philosopher or
psychologist has carefully defined " the given " in this third
sense, he might, in some contexts, find it convenient to speak of
the sensory core *as if* it were literally a constituent of perceptual
consciousness. This policy was recommended, as a matter of
fact, by Josiah Royce, who, like James, specifically rejected the
Sense-datum Theory. Royce recognised the error of confusing
direct inspection with any other procedure, such as perceptual
reduction, in which we merely substitute a new state of con-
sciousness for the one we are supposed to be describing ; states
of consciousness, he says, contain only those elements which on
direct inspection they appear to contain. When we say that a
mental state consists of elements which we ourselves do not
distinguish in it, he says, we may be confusing the mental state
with a physical object, with the brain, with the meaning of the
state in a logical process, " or else, finally, we are referring to a
more sophisticated state of mind which the psychologist, by his
devices for analysis, has substituted for the original and naïve
consciousness ".[2] Nevertheless, he suggests, it may be con-
venient to speak of this " sophisticated state " *as if* it were part
of the original and naïve consciousness. Such a linguistic device,
of course, is quite compatible with the Percept Theory, and may
sometimes be very useful.

[1] A. J. Ayer, *Language, Truth and Logic*, p. 68, Gollancz, 1936.

[2] J. Royce, *Outlines of Psychology*, pp. 109-110, Macmillan, New York,
1903.

2. The Physical Object and the Ostensible Physical Object.

Assuming, then, that there are at least three senses in which the distinction between the given and its interpretation may be recognised by those who accept the Percept Theory, and that there is only one traditional sense in which this distinction must be denied, we are now in a position to consider some of the epistemological questions raised by the statement that *physical* objects are directly given in perception. This statement, as we have seen, has been made repeatedly in recent years by supporters of the Percept Theory, and in one natural interpretation its implications are indeed revolutionary, and probably incredible ; I presume, in fact, that many epistemologists have dismissed it without further ado on the assumption that it is based on a simple confusion of physical objects with *ostensible* physical objects. But the issue is much more complex than such an explanation would suggest.

(a) *The Epistemological Status of Ostensible Properties.*

If any of the philosophers who have said that physical objects are directly given in perception have simply failed to recognise the difference between a physical object and an *ostensible* physical object, then they have, of course, committed a fallacy of some magnitude. To demonstrate this fact with thoroughness by making an exhaustive catalogue of the common properties of physical objects, would lead us into metaphysical questions which are beyond the scope of this paper ; and even an attempt at this point to find a minimum basis of agreement concerning the correct analysis of the term " physical ", would distract attention from the principal issue. But anyone who accepts the Percept Theory must admit that there is at least one important fact about an *ostensible* physical object which serves to distinguish it sharply from a physical object—the fact, namely, that *some* of its properties, if not all, can be discovered by direct inspection of a single state of perceptual consciousness.[1] Whether this is taken to be an epistemological fact or an ontological fact or both, will depend on one's general theory of the mind, but it is

[1] I say " some " because it is possible that a philosopher who accepts the Percept Theory might agree with Broad that things which are present to consciousness " cannot appear to have properties which they do not really have, though there is no reason why they should not have more properties than we do or can notice ". *Scientific Thought*, pp. 243-244.

a fact which cannot be denied by those who accept the Percept
Theory without rejecting the method of direct inspection and thus
the very evidence on which that theory is based.

It is equally certain, on the other hand, that whatever we
may mean by " physical object ", a physical object is at the
very *least* a thing which transcends any one of the states which
might be called a perception *of* it.   This is admitted, of course,
even by Berkeley and the contemporary realists who have
defended various forms of epistemological monism ; none of them,
so far as I know, has maintained that to attribute a property to a
physical object is *merely* to attribute that property to what is
presented in some *one* state of perceptual consciousness.   In
order to confirm a statement about a physical object we may
indeed require the information that can be obtained by direct
inspection of a state of perception, but we also require other
information—information, for example, about the relationship
between this particular state of perception and other experiences,
either actual or possible.   And if this point is obvious to those
who defend epistemological monism, it is undoubtedly still more
obvious to those who accept some form of epistemological
dualism.   Whether, therefore, our general concept of physical
object is in some sense " derived from " the presentation of
ostensible physical objects in perception, or whether it is in one
or another sense " a priori ", the indisputable fact remains
that we do possess two concepts corresponding to the terms
" physical object " and " ostensible physical object ".   And the
difference between these two concepts is sufficiently proved, for
present purposes, by the fact that properties of ostensible
physical objects can be discovered by direct inspection of a
single state of perceptual consciousness, whereas properties of
physical objects cannot.

The fact that the properties of these two kinds of object
are designated by the same *names*, should not be allowed to
obscure this difference in the epistemological (and, for most
philosophers, the ontological) status of the properties.   There is,
of course, some relationship between the properties of physical
objects and the properties of ostensible physical objects which
accounts for the fact that the word " square ", for example, which
designates a property of physical objects, is also used to designate
a certain property of *ostensible* physical objects.   Philosophers
disagree, of course, about the nature of this relationship, just
as they disagree about the number of words in our language
which can properly be used in both the phenomenal and physical
contexts.   But they all agree, so far as I know, that a dis-

tinction may be made between the phenomenal use and the physical use of certain adjectives, and that this distinction reflects an important difference in the status of the designated properties.

(b) The Ostensible Physical Object and Naïve Realism.

Now although it is very unlikely that a philosopher who asserts that physical or material objects are *given* in perception has committed the fallacy of confusing his concept of a physical object with his concept of an *ostensible* physical object, it is a good deal more likely that he has committed a fallacy somewhat similar to this. For he may have assumed that there is no difference at all between his own concept of an *ostensible* physical object and the *naïve* or *popular* concept of a " real " physical object. He may believe, in other words, that what the man in the street means when he says that the paving stones are grey, is precisely what he, the philosopher, would mean if he said that the *ostensible* paving stones are grey. This possibility is suggested by the frequency with which advocates of the Percept Theory describe that which is given in perception as a " naïve world ", a " pre-philosophical world ", a " common-sense realistic world ", etc. And it might help to explain, at least in some cases, what is meant by the statement that physical objects are *given*. For if the concept of an ostensible physical object were identified with the naïve concept of a physical object, such a statement would mean simply that physical objects, in the *popular* sense of the word " physical ", are the directly presented objects of consciousness in ordinary perceptual experience.

If such a statement is made by a philosopher who accepts the Percept Theory, and is intended to express one of the important implications of that theory, it cannot be lightly dismissed as true in any trivial sense. There are some possible interpretations of the word " given ", of course, according to which it may be quite obviously true that physical objects, as popularly conceived, are given to the man in the street during perceptual experience. Thus it is not unlikely that there is some sense of the verb " to believe " such that the man in the street may correctly be said to *believe*, whenever he is perceptually conscious, that there exists a physical object of a certain kind ; and the word " given " might accordingly be interpreted to mean " believed to exist ". This cannot be the interpretation desired by supporters of the Percept Theory, however, for the Percept Theory, as we have seen, is a theory about the *sensuous*

aspects of perceptual consciousness, and not about the beliefs
which may accompany the presentation of an ostensible physical
object. Indeed the obvious fact that the man in the street
has perceptual beliefs about physical objects, is quite compatible
with the rival Sense-datum Theory, whereas the statement that
physical objects are *given* in perception is intended to be a
criticism of that theory.

The question which now confronts us, therefore, is whether
there is any reason to believe that the *sensuous* objects of per-
ceptual consciousness (*i.e.*, ostensible physical objects) are pre-
cisely what the man in the street thinks of as " real " physical
objects. It is usually true, of course, that at the moment of
ordinary perception the man in the street does not *consciously
judge* that the ostensible physical object is *not* a " real " physical
object, but for that matter neither does the philosopher. Nor, on
the other hand, is it plausible to maintain that either of them at
the moment of perception, *consciously judges* that the two
objects are identical. Ordinary perception is simply not reflec-
tive in a sense which would permit either of these two conscious
judgements, whether or not the necessary concepts are somehow
available. The fact that the philosopher possesses two distinct
concepts corresponding to the terms " physical object " and
" ostensible physical object ", is proved by his ability to dis-
tinguish them *on reflexion* ; if an advocate of the Percept Theory
wishes to show, therefore, that the man in the street does *not*
possess two such concepts, he must do so by proving that the
man in the street *cannot* distinguish them on reflexion.

When the issue is stated in these terms, however, it becomes
quite clear that however naïve the man in the street may be, his
naïveté does not consist in his failure to possess *some* concept of a
physical object as distinguished from an ostensible physical
object. To deny this, indeed, would be to deny that he possesses
any concept of " illusion ", and to imply, therefore, that he is a
naïve realist of a type incapable of understanding, even in some
" popular " sense, what it means to say that an oar looks bent
but is really straight. And perhaps it would be relevant to point
out that the man in the street is usually credited with much more
sophistication than this ; many philosophers ranging from
Berkeley to certain contemporary realists have professed to
speak for him, and although they cannot all have described his
views correctly in every respect, they have all agreed in con-
structing epistemological theories which admit the possibility of
illusion. To possess the concept of illusion, however, is to
recognise, at least implicitly, the very difference between a

physical object and an ostensible physical object which would be most likely to impress a supporter of the Percept Theory—the difference, namely, which is reflected in the fact that the properties of physical objects, unlike those of ostensible physical objects, cannot be discovered by direct inspection of a single state of perceptual consciousness.

It would be a mistake, therefore, to say that physical objects are *given* in perception, if the purpose of this form of expression were to imply that when the man in the street says that the paving stones are grey he is talking about what the philosopher would call *ostensible* paving stones. It might seem important to point out that there is some *similarity* between an ostensible physical object and a " real " physical object as popularly conceived—that words which refer to the so-called " secondary " qualities, for example, can be used to describe both of them— but it would surely be misleading to express this fact by saying that physical objects are *given*. There seems, therefore, to be no purely *semantical* fact about the meaning of the terms " physical object " and " ostensible physical object ", which could justify the statement that physical objects are given in perception. There is, however, a *phenomenological* fact which might make such a form of expression seem appropriate to some advocates of the Percept Theory. Let us consider it briefly.

(c) *The Ostensible v. the Apparently Ostensible.*

This phenomenological fact is the one suggested by Dewey's statement that " it is not experience which is experienced, but nature ", the fact, namely, that ostensible physical objects, as presented to us in perception, do not ordinarily *appear* ostensible. Or, to put the matter in a way which emphasises the linguistic difficulties which are always implicit in such discussions as this, ostensible physical objects are not *ostensibly ostensible*. The object of which we are conscious in perception, as Price has so aptly said, " just dawns upon us, of itself. We look and there it is." [1] Thus a tomato is perceptually presented *as* red, *as* solid, and perhaps even *as* edible, but it is not presented *as* ostensible. Nor, on the other hand, is it presented *as non-*ostensible. The terms " ostensible " and " non-ostensible " both refer to epistemological or ontological characteristics, and are properly applied to an entity, as we have seen, after considering such matters as its accessibility to direct inspection ; they are *not*

[1] *Perception*, p. 153.

phenomenal qualities which ostensible physical objects wear on their faces.[1]

It is difficult to find a terminology which will keep this distinction from becoming obscured, and which will at the same time be convenient for the discussion of problems which have both phenomenological and epistemological facets. Thus although it has been convenient, and I hope not misleading, to say that we are *presented* in perception with ostensible physical objects, even such a statement is not unambiguous. In a purely phenomenological context the word " ostensible " would have to be omitted from this statement, since otherwise it could be interpreted as implying that ostensibility is one of the *presented* characteristics of the object. And questions could also be raised about the word " physical " (or any substitute such as the word "material") for this too, as we have just observed, has epistemological and ontological connotations. But the full force of the difficulty is not felt until we try to dispense with the word " physical " and say instead simply that the objects presented in perception are solid and three-dimentional, persisting through time, possessing causal characteristics, etc. For it will then become evident that each one of *these* descriptive terms also has epistemological and ontological connotations. We have already seen that such terms, when used to describe physical objects, designate properties the existence of which cannot be determined by direct inspection of a single state of perceptual consciousness, and we surely do not want to suggest that in perception we are presented with properties of *this* kind. The pervasiveness of this terminological difficulty, as we have also seen, is a result of the fact that in a phenomenological context most, and perhaps all, of the adjectives in our language have a meaning which they could not possibly have if they were being used to describe physical objects.

It is possible, therefore, that the philosophers who have maintained, on the basis of the Percept Theory, that physical objects are actually *given* in perception, have used this form of expression in an attempt to solve, at least partially, the terminological problem just outlined. This form of expression, as we have seen, does not by any means solve the entire problem, but it might help to emphasise the fact that the objects which are

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[1] It might be argued that an oasis may appear ostensible to an experienced desert traveller who knows that he is experiencing a mirage. But in that case the word " ostensible " has a phenomenal significance which could be absorbed by the statement that the traveller perceives a mirage of an oasis.

presented in perception are not presented *as* ostensible. And some advocates of the Percept Theory have thought it important to emphasise this fact because the failure of epistemologists to recognise it may have been responsible for the generation of " pseudo-problems ".[1] Similarly, the statement that in perception we are " directly aware " of physical or material objects, which is often made by the same philosophers, can perhaps be accounted for as an alternative method of emphasising the same phenomenological fact. Indeed neither of these two forms of expression seems entirely inappropriate when considered in this light, although they may have been the cause, because of their traditional epistemological connotations, of more misunderstanding than they have prevented.

However this may be decided, the issues to be discussed in the following pages are primarily epistemological ; and it is consequently impossible to restrict ourselves to forms of expression which would be appropriate in a purely phenomenological context. I shall continue to say, therefore, that the objects directly presented or given in perception are *ostensible* physical objects, recognising, however, that I can say this only because I am not attempting *merely* to describe the phenomenal properties of the objects of perceptual consciousness—only, in short because I have adopted the epistemological point of view. From this point of view it is clear that physical objects, because they are things the properties of which cannot be discovered by direct inspection of a single state of perceptual consciousness, cannot reasonably be said to be " directly presented " or " given " in such a state of perceptual consciousness.

In this particular respect, of course, the distinction between an ostensible physical object and a physical object is strictly parallel to one of the most important of the traditional distinctions between a *sense-datum* and a physical object ; for sense-data have traditionally been conceived as observable by direct inspection, and physical objects as knowable only through some more complex process. In fact it should now be clear that the concepts of physical object and ostensible physical object are so independent of one another, from a logical point of view, that the basic distinctions will not be affected by the outcome of the conflict between the Percept Theory and the Sense-datum

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[1] *Vide, e.g.*, K. Duncker, " Phenomenology and Epistemology of Consciousness ", *Philosophy and Phenomenological Research*, June, 1947. I believe that Duncker is clearly mistaken, however, if he intends to imply that there is no genuine problem concerning the possibility of transcending consciousness.

Theory. The questions which remain to be answered therefore, in any attempt to evaluate the epistemological implications of the Percept Theory, are questions about the epistemological *functions* of ostensible physical objects ; and the most direct way to raise these questions is to ask whether ostensible physical objects are adequate substitutes for performing the functions which sense-data have traditionally been supposed to perform in the processes by which we acquire and confirm our beliefs about the physical world.

3. Epistemological Functions of the Ostensible Physical Object.

(a) *The Sign Function.*

I believe that there are two such functions, one of them perhaps more strictly psychological than epistemological. The first of these, the psychological function traditionally attributed to sense-data in the knowing process, is that of somehow *determining*, at least in part, the nature of the physical object which the perceiver in some sense " believes " to exist at the moment of perception. It is impossible to describe this function more precisely except in terms of some particular form of the Sense-datum Theory. Thus Berkeley, who accepted what I have called the " discursive inference version " of that theory, was willing to say that the sense-datum given in perceptual experience serves as a " sign " of the existence of a certain kind of physical object (for Berkeley, of course, other " ideas "). Other philosophers who have accepted the Discursive Inference Theory have said that the sense-datum " causes " us to " make a judgement about " or to " think of " a physical object. Such forms of expression are intended to imply that perceptual consciousness is discursive.

Philosophers and psychologists who accept the more common Sensory Core Theory, however, have often found it more difficult to describe the relationship between a sense-datum and perceptual beliefs. Words like " sign ", " clue ", and " cause ", do not seem appropriate for a sensory core which is conceived as occurring simultaneously with the perceptual beliefs. " The best analogy we can offer ", says Broad, ". . . is to be found in the case of reading a book. . . . If there were no print we should cognise no meaning, and if the print were diffcrent in certain specific ways we should cognise a different meaning ".[1]

[1] *Scientific Thought*, p. 66.

This analogy is not very good, however, for the print on the physical page transcends the entire state of perception and might therefore be said partially to "cause" the perception, whereas awareness of the sense-datum is regarded as a *constituent* of the state, and could not be said, in the same sense, to cause the beliefs which accompany it. Perhaps those who accept the Sensory Core Theory should restrict themselves, as Broad suggests, to the statement that the sense-datum and the beliefs are functionally related in such a way that "if this sensum had different properties we should ascribe different properties to the physical object".[1] Or perhaps they might say, just a little more specifically, that sense-data and the accompanying beliefs are both caused simultaneously by certain physical events within and without the perceiving organism.

Now the Percept Theory, as we have seen, does not admit the existence in ordinary perception of a temporally distinct sensuous constituent ; ostensible physical objects, therefore, cannot fulfil the function of a perceptual "sign" as conceived by Berkeley and other exponents of the Discursive Inference Theory. This is probably only a matter of academic interest to most contemporary epistemologists, however, for they have apparently rejected the Discursive Inference Theory in favour of the Sensory Core Theory. But if it is true, as those who accept this latter theory maintain, that there is *some* functional relationship between the sensuous constituents of perceptual consciousness and the beliefs which accompany them, this relationship can quite consistently be recognised by supporters of the Percept Theory. In whatever sense it is true, in other words, that consciousness of a sensuously clothed ostensible physical object is accompanied by beliefs about the existence of a real physical object of a certain kind, in that sense it is meaningful to say that the beliefs are functionally related to the characteristics of the ostensible physical object. And thus we could even say, directly paraphrasing Broad's statement about sensa, that perceptual beliefs are "based upon" ostensible physical objects in such a way that if a particular ostensible physical object had different properties we should ascribe different properties to the physical object. So far as the traditional *psychological* function of sense-data is concerned, therefore, the Percept Theory gives rise to no problems

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[1] *Ibid.*, p. 247. *Cf.* Price : The ostensible object " is forced upon me by the character of the sense-datum . . . and no other ostensible object but precisely this one could ostend itself to me here and now, the sense-datum being what it is ". *Perception*, p. 148.

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which are avoided by the Sensory Core Theory ; and to carry the discussion beyond this point is unnecessary for the present purpose.

(b) *The Function of the Ostensible Physical Object in Confirmation.*

The second, and more strictly epistemological, function traditionally attributed to sense-data, is that of serving as an important part of the evidence to which we must appeal in any attempt to *justify* our beliefs about the physical world. The statements which express these beliefs, according to one of the most familiar strains of traditional empiricism, can be divided into two groups : the first includes only singular statements about physical objects or events, and the second includes all other statements about the physical world.   Statements in the second group, according to this theory, can be justified only by an argument the premisses of which include statements in the first group. And statements in the first group can in turn be justified ultimately only by an argument the premisses of which include statements about sense-data.   Thus sense-data have been regarded by many philosophers as the very foundation stones of empirical knowledge, without which a rational construction of physical science would not be even theoretically possible.

For present purposes it is unnecessary to consider the points of disagreement within this well-known school of empirical thought. There has been disagreement about the proper analysis of the statements which refer to sense-data, about the necessity for additional premisses, about the number and variety of principles of inference required to draw conclusions about physical objects, and about the epistemological status of such principles of inference ; but these differences may be disregarded without prejudice to questions about the general function traditionally attributed to sense-data. I shall say, for convenience, that all members of this empirical school believe that sense-data are " epistemologically basic " ; and the problem which immediately concerns us, therefore, is whether the Percept Theory has any implications for the theory that sense-data are epistemologically basic. This problem is sufficiently important to deserve careful consideration even by the many philosophers who are convinced that beliefs about physical objects cannot be justified except in some *relative* fashion by reference to *other* beliefs about physical objects.

Now among the singular statements which express our beliefs about physical objects, there are some which express *perceptual* beliefs, *i.e.*, beliefs which are entertained at a time when we are presented with an ostensible physical object, and which are, as we have said, in some sense " based on " the ostensible physical object ; such beliefs are usually expressed by statements similar in form to the statement " This (or that) is a tomato ".

Perceptual beliefs could probably be described dispositionally by reference to tendencies towards bodily behaviour or towards conscious experiences, or both ; though perhaps, as we have already observed, some philosophers might wish also to make some reference to *phenomenal* events occurring simultaneously with the presentation of the ostensible physical object.

It is convenient, in considering the epistemological implications of the Percept Theory, to begin by paying special attention to those singular statements which express non-perceptual beliefs about physical objects but which are commonly supposed to be justifiable by reference to *past* perceptual experience.   Thus we might consider, for example, the statement " There is a tomato behind me ", with the understanding that it expresses the present belief of someone who has recently been presented with an ostensible tomato but who is now looking in another direction. Such beliefs, of course, constitute a considerable proportion of all our beliefs about the physical world.

Most philosophers who accept the Sense-datum Theory and who also believe that sense-data are epistemologically basic, would probably maintain that in such a case the belief expressed by the statement " There is a tomato behind me ", could be justified to some extent by means of an argument based on the present memory of past sense-data.   Indeed they *must* defend a position of this sort if they are to bring their theory even roughly into line with common-sense, for the fact that someone has recently had a perceptual experience of a kind that he associates with the existence of a tomato, *is* commonly regarded as epistemologically relevant to his present non-perceptual belief in the existence of a tomato.   But if the Percept Theory is true, this non-perceptual belief about the tomato could almost certainly *not* be justified in this way.   For assuming that the recent perception of the tomato had not been perceptually reduced for psychological or aesthetic purposes, it would simply be false to say that the observer had been aware of any sense-datum at all.   The only sensuous constituent of that past perception would have been an ostensible tomato, which, as we have seen, is quite different from a sense-datum as traditionally conceived.   If

the present belief is to be justified by reference to *anything* that has been sensuously given, therefore, it must be justified by reference to the ostensible tomato. And it is just this phenomenal thing, indeed, that the normal observer *would* remember under such circumstances ; he would remember that he had been presented with a full-bodied tomato with all its sensuous qualities of redness, smoothness, warmth, and sweetness, and not that he had been aware of a round red patch, or perhaps, depending on the lighting, a patch of some quite different colour.

The traditional analysis of justification by reference to sense-data is somewhat different, however, when the sense-data in question are conceived as *future* rather than *past* objects of direct awareness ; and because of this difference it is not sufficient for those who support the Percept Theory to point out that sense-data do not occur in *ordinary* perceptual experience. According to the philosophers who believe that sense-data are epistemologically basic, the statement " There is a tomato behind me " could be justified to some extent not only by reference to past experience, but also by turning around and inspecting the new sense-datum which is thereupon presented. And in a procedure of this kind it might indeed be possible, by adopting the reducing attitude, to produce a pure state (or approximately pure state) of direct awareness. And the sense-datum which is thus presented might possibly be used as the basis of an argument to justify the original non-perceptual belief about the tomato. It would be presumptuous indeed, for those who support the Percept Theory to maintain that such a procedure is *never* followed in an attempt to justify a belief about a physical object.

It would be quite unrealistic, on the other hand, to maintain that such a procedure is *usually* or even *frequently* followed. The procedure which is usually regarded as confirming a belief about a physical object involves various operations (*e.g.*, manipulation of the object) but it does *not* involve the operation of introspective reduction. The usual procedure, as Price has aptly described it, is that of " specifying the unspecified ". We look at the tomato, for example, from several points of view, turn it over in our hands, squeeze it, etc., and by these means produce a series of perceptual states. The ostensible physical object remains a tomato throughout the entire process, but the tomato becomes progressively more determinate, more specified, in each succeeding perception. And as the relatively unspecified tomato becomes more and more specified, so we at the same time become more and more convinced that our belief has been justified. There is obviously no need to refer to sense-data

in describing this process, and if we were to formalise this common method of confirmation we should have to treat the series of ostensible physical objects, and not sense-data, as epistemologically basic.

There is also another fact which shows that if the Percept Theory is true sense-data cannot be regarded as epistemologically basic without doing violence to common-sense. This is the fact that we are so often surprised, after we adopt the reducing attitude for psychological or aesthetic purposes, at the characteristics of the sense-data which are thereupon presented to us. The fact of the matter is that most of us are simply not prepared, in many cases, to predict the characteristics of the sense-data which we can produce by perceptual reduction ; we are not prepared to say, for example, what the colour of our sense-datum will turn out to be if we are looking at a field of green grass on a cloudy day, or in the late afternoon when it is lighted by the rays of the setting sun. We are simply not familiar enough with the relationship between the physical stimulus and the conditions of observation on the one hand, and the sense-data which are the products of perceptual reduction on the other hand. Because of the psychological fact of object-constancy, however, the qualities of ostensible physical objects are more easily predicted ; the ostensible grass, for example, is likely to be green whether the sky is clear or cloudy. However the epistemological relationship between physical objects and ostensible physical objects may be conceived, therefore, it is apparently better understood on the practical level than the relationship between physical objects and the relatively rare objects of direct awareness. This in itself seems to be a good reason for putting greater faith in a process of confirmation in which ostensible physical objects, rather than sense-data, are treated as epistemologically basic.

If the philosophers who believe that sense-data are epistemologically basic are not interested in bringing their theory into line with common practice in the justification of belief, they might still insist, to be sure, that perceptual reduction is necessary for " proper " confirmation of beliefs about the physical world. But it is difficult to see what could be said in support of such a position, unless an appeal were made to some ontological theory about the *constitution* of physical objects, in particular to a theory that physical objects are in some more or less literal sense " composed of " sense-data. Such theories, however, would lose whatever plausibility they may have if they were not themselves supported by epistemological considerations ; and if the

Percept Theory is true, indeed, these theories must themselves be revised if they are to retain the epistemological advantages traditionally attributed to them. To many philosophers this fact will undoubtedly seem to be one of the most important consequences of the Percept Theory, and we ought to consider it briefly before concluding our examination of the epistemological implications of the Percept Theory. Let us now do so.

4. THE PERCEPT THEORY AND EPISTEMOLOGICAL MONISM.

Epistemological monists from Berkeley to many contemporary realists have used the theory that sense-data are epistemologically basic as a premiss in their attacks on epistemological dualism. Knowledge of physical objects is possible, they have said, only if statements about physical objects can be construed, in some more or less complex manner, as statements about sense-data—or only, as some of them have preferred to say, if physical objects are somehow " composed of " sense-data. Their analysis of physical statements has not usually been based entirely on epistemological considerations, but in most cases their analysis was at least *suggested* by these considerations.

If the Percept Theory is true, however, the epistemological advantages traditionally attributed to monism can be retained or ly by reinterpreting statements about the physical world in terms of ostensible physical objects and events instead of sense-data. For one effect of the Percept Theory, as we have seen, is to change the denotation of the term " epistemologically basic ". In whatever sense, therefore, a philosopher wishes to maintain that physical objects are " composed of " things that are epistemologically basic, in that sense he must say, if he accepts the Percept Theory, that physical objects are " composed of " ostensible physical objects. I do not propose to evaluate the results of such a reinterpretation but only to indicate what form it must take, and in particular to point out that ostensible physical objects can vary in ways that sense-data cannot, so that a new problem is uncovered as soon as we attempt such a reinterpretation.

To state this problem clearly let us say that according to the traditional forms of epistemological monism every physical object is a " family " of sense-data [1] If we do not specify the ontological status of sense-data—whether, for example, they can exist unsensed and whether they may be " hypothetical "—

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[1] The term, of course, is the one used by Price for a collection of sense-data unified in a certain way. *Vide, Perception*, p. 227.

this statement can be interpreted as expressing a point of agree-
ment among epistemological monists whether they be called
"idealists", "realists", or "phenomenalists". For a family
of sense-data would simply be the class of all those sense-data,
actual or possible, which would have to be mentioned in making
a complete ontological analysis of a particular physical object.

Now according to the traditional Sense-datum Theory, as we
have seen, the sense-data observable by any one sense are quite
limited in their qualities ; visual sense-data, for example, may
vary only in shape and colour.   If the shape and colour of a visual
sense-datum remain unaltered throughout a given period of time,
therefore, changes in the attitude of the observer during that
period cannot be said to affect the sense-datum at all.   In the
case of ostensible physical objects, however, the case is quite
different ; the colour and shape of two ostensible tomatoes may
be exactly similar although the ostensible tomatoes, because of
changes in the attitude of the observer, are quite different in other
respects.   To a hungry man the tomato may be presented as
warm and sweet and edible whereas to someone looking for a
missile it may be presented as soft and juicy and just about as
heavy as a baseball.   Thus it is clear that a family of ostensible
physical objects is even more numerous than a family of sense-
data, and that the relations among its members are many times
more complicated.   As a matter of fact it might even be more
appropriate to say that a physical object is nothing less than a
nation of sense-data, the nation in its turn comprising as many
families as there are attitudes capable of affecting the content of
perceptual experience.   Thus the various perspective views
of a tomato which are obtained by walking around it, could be said
to be members of one family provided that they are all determined
by the same attitude.   And by walking around the tomato a
second time, but with a different attitude, the observer could be
said to become acquainted with members of a second family.
And so on.

A philosopher who wished to maintain such a position, however,
might not feel obliged to hold that members of all these families
must be mentioned in an ideal translation of *every* statement
about the tomato in question.   He might insist, and with con-
siderable plausibility, that what the hungry man *means* when
he uses the word " tomato " is likely to be quite different from
what the man in search of a missile means when he uses the same
word, and that this difference in meaning must be reflected, in an
adequate analysis, by the choice of families to be represented in the
translation.   Thus there might often be cases in which the

meaning of the statement " This is a tomato ", if u~~~ ~~ ~~~ss the limited belief of a particular observer, might be adequately translated into statements about ostensible physical objects which belong to very few families within the nation, or perhaps to only one. This is not the place to examine such possibilities more fully ; it is sufficient for present purposes to point out that the Percept Theory creates a new problem for the epistemological monist, but that this new problem does not appear insoluble.

5. CONCLUSION.

We may finally conclude, therefore, that there are at least four important epistemological implications of the Percept Theory : (1) The traditional psychological distinction between the given and its meaning or interpretation, must usually be construed as a distinction between the ostensible physical object and certain accompanying events, either phenomenal or physiological or both. (2) There is one traditional meaning of "the given", however, for which there is no simple substitute in terms of the Percept Theory; for this particular meaning a more complex substitute may be provided by means of a " definition in use ". (3) The denotation of the term " epistemologically basic " must be understood to be ostensible physical objects rather than those thing₃ which have traditionally been called " sense data ". (4) As a result of this fact all the traditional forms of epistemological monism must be reinterpreted to make physical objects " nations " of ostensible physical objects rather than " families " of sense-data.

On the other hand the Percept Theory has no implications concerning the general epistemological or ontological status of physical objects.   It does not imply that some form of epistemological monism must be correct, nor even that there is some reasonable sense in which it would be true to say, in an epistemological context, that physical objects are " directly presented " in perception.   Those philosophers are mistaken, therefore, who have inferred from the Percept Theory that the traditional problems concerning the epistemology of perception are pseudoproblems, or that they must be completely recast to make them fit the phenomenological facts on which the Percept Theory is based.

The epistemological implications of the Percept Theory, we may conclude, are important but not revolutionary.

<div style="text-align:right">RODERICK FIRTH.</div>

Swarthmore College.

IV.—EMPIRICISM, SENSE DATA AND SCIENTIFIC LANGUAGES

By A. C. Lloyd.

For most empiricists epistemology means giving rules for the construction of sentences which, when true, are immediate records of experience or of states of affairs. These statements may be called " correctors ", because they are the statements by which other statements are verified and not *vice versa*. In logic they have been called " atomic " because their structure is simple compared with that of other sentences. It is the aim of this article to show that correctors are always relative to a language ; that is to say, they are absolutely incorrigible only in the sense that there are no sentences which can correct them in the language to which they belong [1] : but they are *not* absolutely incorrigible in the sense that there is only one language which can truthfully describe the world or which embraces all other languages. Accordingly it will be contended that those empiricists who claim some primacy for a sense datum language are wrong. What has made them go wrong, it will be argued, is an erroneous belief that because observation-statements are statements of observation these statements themselves presuppose (or even state) that they are statements of observation. A theory of knowledge, in my view, is a choice of languages. For theories of knowledge differ by giving different rules for constructing correctors. But " sentence S is a corrector " means " S is incorrigible in language L ". And this statement asserts nothing about the nature of experience, but asserts something only about a language in which to describe it ; otherwise the existence of a language M, in which S is not incorrigible, leads to a contradiction. There is therefore a choice of correctors, which is technically the choice of a language.

What does it mean to say that there is a choice of languages ? Nothing very startling. A language, as studied by logic, is a system of symbols, *i.e.*, symbols which obey certain rules. (When the language is *used* or the symbols " have an interpretation ", they obey semantic as well as syntactic rules.) But such a system is an abstraction from something which can be studied by

[1] " There is a sentence, S ", means " S is a significant expression " or " is allowed by the formative rules ".

the empirical scientist, namely habits of behaviour. For this reason a language, as the term is used here, consists of symbols, not all of which are necessarily words ; we must not confuse the *sign-vehicles* (" dinner ", " déjeuner ", etc.), which are irrelevant here, with their *meanings*, or the *symbols*, which are behaviour patterns (salivating, etc.) [1]. This is important, because it is commonly supposed that adherents of the view I am defending think that they are changing a language by changing the sign-vehicles. But the point I want to make is that, as habits of behaviour, any symbols or meanings in use, like any other states of affairs, *could have been* different from what they are. And it is in this sense that there is a choice of them, and therefore a choice of languages. There is not just one possible system. Of course our habits are subject to causal determination, and the " choice " is generally only a *logically possible* choice. But philosophers can make the choice consciously, as Berkeley chose a sense datum language and his critics another.

This way of looking at theories of knowledge is, like the theories themselves, only one among alternatives. What then is its advantage ? It makes statements about the nature of evidence linguistic rules, and isn't that misleading ? Certainly it is misleading, for they are more than linguistic rules—but in the sense that there is a lot more to be said about them than that ; and this is a form of misleadingness from which all description suffers. By showing, however, that that is their logical status we remove epistemology from " metaphysics " or Platonism ; that is, we deter people from what may be called " Naaman's fallacy "—the fruitless search for some absolute or unique theory of knowledge. It happens, moreover, that we can talk of " showing " that that is the logical status of correctors, rather than of " choosing a system in which that is their logical status " ; for the contemporary philosophers with whom I shall be concerned are committed by their presuppositions, I believe, to such a system ; this I hope to show in the first part of my argument.

The argument falls into two parts : (i) Correctors are relative to a language. (ii) There is no primacy possessed by a sense-datum language ; for (*a*) alleged proofs of the primacy are fallacious, (*b*) there is a valid alternative language (" the physical object language "). The connexion between the two parts is this : (ii) is deducible from (i) ; but many philosophers believe

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[1] Although the behaviour-patterns will include also the use of the sign-vehicles as the production of certain noises or written shapes.

that there are good grounds for accepting (ii) and so, I think, are frightened off (i) : it therefore seems best to argue their merits separately.

The Relativity of Correctors

Take some version of the language of sense data or sensibilia. A (molecular) sentence about a physical object is here equivalent to a set of (atomic) conjunctions about sense data—but only under certain conditions. We cannot say that " This is water " means (among other things) " There will be bubbly sense data at the same time as coinciding sense data of black line marked 100 [on a centigrade thermometer] and grey line [top of mercury] ". For suppose we poured the stuff down the drain, the latter proposition would be false, but the former proposition, " This is water ", could be true. We have therefore to add to the definiens a proviso equivalent to " if I boil it ". And this need to state conditions occurs whenever there is to be a statement of *meaning*. In fact a statement of meaning is an implication-statement.

Now many empiricists would admit this in the case of physical object sentences, but have not realised that it must apply also to their sense datum sentences. For if we deny that the latter are also equivalent to certain implication-statements, we are either denying that they have any meaning or we are committed (as Schlick was) to a theory in which we know the meaning of " This is red " only in the act of knowing that this is red. In one case a significant science would be built out of insignificant reports ; in the other case only true statements would be significant. It follows that sense datum sentences are not atomic in some absolute way. Moreover, being equivalent to various implication-statements of a sort similar to the definitions of physical object sentences, they will, like physical object sentences, be found to imply sentences as yet uncorrected (but corrigible). But empiricists who look for a form of certainty which is absolute or independent of language consider sense data sentences to be correctors because they are absolutely incorrigible. This our argument shows they cannot be.

The correct and only solution is for the empiricist to state that certain sentences (whether some or all sense datum sentences) will be correctors in his system ; any uncorrected sentences which they imply will then be treated as though they had been corrected. And this will be a rule of his system or language, although any reasons for preferring it will naturally be extra-linguistic.

It must be emphasised that I am not attacking a sense datum

language in itself, but only what its supporters have commonly claimed for it. They have ignored the dilemma between whose horns they should have to choose—the dilemma of corrigibility or insignificance. One reason for this is that when they have seen a puzzle over meaning they have tended to see it only in terms of " privacy " or the problem of solipsism. This is irrelevant. When Jean-Jacques argued with Rousseau, they had to understand each other. But in ignoring the dilemma they have tacitly presupposed the correct solution. They have treated the statement naming their correctors as a necessary statement : only many of them have not recognised that its necessity is relative to the language which they are advocating.

Indeed these arguments are sometimes used as part of a case against the existence of correctors at all. To discover the truth or falsity of any assertion, it is objected, it is necessary to discover its coherence or incoherence with all other assertions. This is a more difficult controversy, to my mind, than the protagonists on either side generally realise. The result is that most of what both have said can probably be maintained, for they seem to me to be conducting analyses at different levels. In addition, words may be defined in as many ways as there are languages ; there are as many languages as there are consistent sets of syntactic definitions ; there are as many usable languages as there are consistent sets of semantic or ostensive definitions ; " true " is a word (even if not so easy to use as " horse "), and it is thus unlikely that there is only one usable definition of it. I do not wish to burke this in presenting the following argument.

One of the standards by which one language or theory of knowledge is to be preferred to another is its suitability to contemporary scientific method. (The reader must allow the term " suitability " to be left undefined.) And this criterion leads us to re-define " correctors " in these days as " atomic sentences with the highest initial probability when asserted ". Now it is worth considering whether the greater probability of certain sorts of statement, e.g., the scientist's single observations, could be analytic—deduced for instance from the classical theory of probability on the ground of the inverse variation of certainty and prediction ; for the chances of contrary assertions might be considered less for the consequences of a sensible and therefore relatively simple predicate than for the consequences of a so-called physical and complex predicate. This would depend, however, on there being differences in the degree of ramification of consequences within a system ; and such differences would not

be possible in the Laplacean system assumed. None the less there is still a valid sense in which ramification does vary, although the variation will only appear in a language which employs functors, such as length and temperature, whose values are quantitative. The assertion of an earthquake in Rome implies, not more sentences, it is true, than the assertion of a bear's sneeze at the North Pole, but at least sentences containing functors whose values are enormously greater. It must not be forgotten that this peculiarity, which is reflected in inverse laws of distance, is what enables science to advance at all. But unfortunately we find that it cannot be used by a coherence theory in the way I have suggested. It is an empirical peculiarity about the world ; the assertion of it would certainly not be a tautology. Its use to justify the greater probability of empirical statements would therefore be a circular argument. I conclude that there is only one way in which a coherence theory can be made suitable to scientific method (though this is not the same thing as being tenable). It must admit some restriction on ramification of consequences as a postulate or some other conventional means of ensuring a greater weight to empirical statements. And this, I suggest, has been done tacitly or openly by most of its subscribers. But by so doing it has reintroduced correctors to the language, according to our modified definition of correctors as initially most probable sentences. These may be named by deduction from a postulate, but they are then as relative to a language as the postulates.

NOTE : It will have been noticed that the argument from suitability has been over-simplified ; but the principle holds good. Neither a language with correctors nor one without appears suitable to actual scientific practice. It is not necessary there to name correctors, but it is necessary that there should be the possibility of naming correctors.

Alleged Primacy of the Sense Datum Language

The ground covered in reaching the first general conclusion may be familiar. But many philosophers accept the position only to undermine it. They say (though not necessarily in this terminology) something like this : " Yes, rules about correctors are relative to the language concerned. But whatever other languages there may be, a language of observation possesses a certain primacy." Epistemologically, such a language cannot be of equal status with the others. This is what I called " Naaman's

fallacy " ; and the denial of it is the second general conclusion I wish to draw. It will first be suggested that the arguments for the primacy of an observation language are invalid ; and as this is not by itself sufficient refutation the nature of an alternative language will also be described.

As for the language of observation—the language traditionally associated with empiricism—I shall continue to refer to it as the sense datum language. But it must be emphasised that the name is to apply to any version of it—no more is meant than what a German philosopher might call a language of experience. It does not matter here whether we perceive zebras or two-dimensional, striated patches. Moreover, nothing metaphysical is implied ; it is immaterial whether the version is phenomenalist or realist. The alternative language with which it is to be compared is that of science ; and whether this is to be called a physical object language or a scientific object language should depend on the partly empirical question whether biological sciences are reducible to physics. We can ignore the point.

Now it is, I believe, possible to see that any argument for the priority of sense datum predicates to scientific object predicates is circular. How this has been missed by many philosophers must be considered in a moment. Let us start from a common, but relatively unsophisticated, argument. This runs, " Whether or not a sense datum sentence or language is reducible to a physical object sentence or language is beside the point ; for you cannot get away from the fact that your only knowledge of physical objects is by means of sense data (or experiences). The sense datum language is thus epistemologically prior." There is no need to get away from the fact—but only to point out that the conclusion does not follow. If it is a fact, it means roughly that the scientist depends on the vagaries of his sensations for his knowledge. And the realisation of this might causally influence him to choose a sense datum language : but no statement about his choice of a language is deducible from such a fact. If it is a tautology, it seems to define knowledge as knowledge by observation and so to mean no more than, " You cannot observe x without observing x ". This truism can be made to sound more important if we pronounce it, " You cannot observe x without *observing* x ". But the choice of a sense datum language would only follow from it if statements of observation not only *were* statements of observation but *stated* that they were. Now although it has sometimes been held that they ought to state it, it is a mistake to suppose that they even *could* state it. For the statement " ' p '

is an observation-statement " belongs to a meta-language of
" p ". For the same reason it cannot be *implied* by " p " ;
and this fact of logic rules out a misleadingly expressed ground on
which I suspect a sense datum language may have been thought
necessary—the ground that it is a " presupposition " of scientific
statements that they are statements of observation. (The
word " presuppose " is to be recommended when one is not
sure whether it is safe to use " imply ".)

This version of Naaman's fallacy is one which Berkeley and
certain members of the Vienna Circle were pretty clearly guilty
of. It is, so to speak, the prototype of the trap. But the philos-
ophers I want now to argue against have recognised it. They
have succumbed to a more refined, because more disguised, model.
If they insist on a sense datum language, it is (I maintain) on
account of the fact that a sense datum language is used as the
language of immediate observation. I report what I sense by
saying, " I have toothache ", " I hear a certain musical note ".
I do not normally say, " There is an electric impulse of a fre-
quency greater than so and so in the smallest nerve fibres con-
nected with Lloyd's teeth ", or " in a nerve connected with a
certain fibre of Lloyd's basilar membrane ". That my report
takes that form, however, is just a contingent fact. But this fact
can only be stated in a meta-language of the report. Logically
therefore the fallacy will be the same as the cruder type.

I suggest that the process of thought involving the refined type
of the fallacy may be schematised as follows.

(1) *Sense data, such as aches and noises, are described by sense
datum words, such as " aches " and " noises ".* This is elliptical
for " . . . *are described in the class of languages* λ " (say, " . . .
all languages known to me "). It implies

(2) There are sentences about sense data.

Both (1) and (2) are true, and they are compatible, it is
admitted, with

(3) *There is a language L which contains no sense datum words.*

But since (2) tells us that there must be significant statements
about sense data, we are compelled to infer

(4) *Sense data are described in L by non-sense datum words*
(*e.g., physical object words*).

The position now is that we have a possible language L which
in one way is not a sense datum language. It is an alternative
language in the same sense that German is an alternative to
English. English says " toothache ", German says " Zahnweh ",
L says " electric impulse in the smallest nerve fibres connected
with the teeth ". In fact we can construct a language in which

there are no sense datum words, but we cannot, without contradicting (2), construct one *in which there are no sentences about sense data*. So in German there is no word " toothache ", but there are sentences in German about toothache. And given a language in which there are sentences about sense data, it is not to be denied that there is a valid sense in which they are prior to other sentences. When an alternative language to that of sense data was promised, it was necessary to produce one *in which there are no sentences about sense data*. And this is just what appears impossible. So far from being fallacious, " Naaman's fallacy " seems to have been proved correct at least in this version : in whatever *words* it talks, every language talks *about* sense data.

The conclusion is not without interest inasmuch as it is a special case of a general objection which many seem to feel against the virtual reduction of philosophy to the study of languages and the conventionalism which apparently follows. " There must be *the* solution to the problem, *the* analysis of the concept—*the* language, if one must use your terms." So does some inner voice affirm. " The solutions remain unchanged : only the conventionalist does not see that his different languages make no more difference to them than the Latin or the French in which Leibniz chose to address Arnauld made a difference to the theorems he was expounding." One is reminded of the apriorists' insistence that so-called alternative logics are contrived only by transposing symbols in a truth table.

What then was wrong with our opponent's argument ? Simply this. He probably saw in (1) that the phrase, " described by sense datum words ", was incomplete because it is a semantic phrase and must have reference to a particular language. But he failed to see that the whole sentence itself belongs to a language ; this is important since " sentences " is a semantic term in (2) and must have reference to a particular language, while his argument depended on the unconditional character of (2), *i.e.*, it was taken as a straightforward existential statement. This has the same result as taking it to belong to the language of (1) —the result, namely, of making (4) a valid conclusion. But if M is the language in which the argument is conducted, *i.e.*, a meta-language of λ, (2) belongs to a meta-language of M and is true of M and of λ : but only by a *petitio* can it be assumed that it would be true of L.[1]

[1] Here too some strictness is sacrificed to simplicity. Only a rather complicated device would make it possible to state the argument at all, apart from the *petitio*.

All this represents merely the difficulty of describing a novel language if we only have old languages in which to describe it. As a matter of fact the position of the scientific object language is not so bad as that.   To a greater extent than any other language it does already exist.   But it is rare among philosophers.   There are some philosophers (*e.g.*, phenomenalists and neutral monists) who are willing to regard it as a sub-language of the sense datum language ; and there are some (dualists) who are willing to regard the sense datum language and the scientific object language as two mutually exclusive sub-languages composing some comprehensive language.   But few allow it to be independent.   It may be asked what is the advantage of this pedantry.   The answer is that the imitation of mathematics in philosophy has always been meant as a challenge to one's opponents—a challenge to show either that his argument is invalid or that their argument is not covered by it.   In the case of our problem, moreover, when terms more traditional to epistemology are used, the *petitio* is so deeply embedded that discussion becomes impossible.   For statements about sentences and meanings then relapse into statements of existence and the possession of descriptive attributes, that is, they have the form of statements about things.   (2) becomes simply " *There are sense data* " or some equivalent and the relativity is completely disguised ; for to add any condition except an existential one would be meaningless.   How is such an assertion possible ?   It is clearly not of the same sort as " There are black marks here " or " There are zebras ".   " By pheno-menological observation ", the modern philosopher might claim, that is, by examination of " experience ", of " the given ". Whether this is said to be direct or indirect observation (inference) does not matter.   For " experience " or " the given " means for our purposes " sense data ", and it is the possibility of asserting their existence which is in question.   Of course, the question is really one of description, not of fact ; but still thinking in ontological terms we can see that no *evidence*, let alone proof, of *metaphysical* sense data is possible for those who do not already accept them.   Those, whether monists or dualists, who cling to the method of phenomenology or introspection will never see this because their method is putting a premium on—indeed, making an absolute of—that vertical chain of language and meta-languages which, as a contingent fact, they use.   For languages are real in the way that whist is a real habit (among Englishmen, but not Eskimos), as well as abstractions in the way that whist is an abstraction as studied by someone who wishes to state what rules it has in common with auction bridge.   And there are

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always two sorts of " must " about a choice of language or definition. We *must* choose one rather than another because of a word's pragmatic (intuitive, sensuous) meaning—the sense in which " tramcar " *means* different things to a Londoner and an educated Eskimo. This " must " concerns pragmatics and the suitability of language, which involves many empirical considerations. In the second place we *must* choose one definition rather than another because of a word's syntactic, well-defined meaning ; this simply refers to the well-known fact that a dictionary is a closed system ; if x, y, z have been used in the definition of a, we shall not be free when we come to it to define z as we please. Naaman's fallacy tries to make the first kind of " must " a case of the second. To do this is to suppose that one language can be the uniquely true or correct one, and that can only be done by assuming some realm of essence, a world of real meanings which duplicates the world of things (including words). Plato, we know, assumed it. What seems to have happened in modern philosophy is that the majority have rejected Socrates' supposition that there were some absolute definitions of " hot " and " cold " and even " brave ", which only the philosophic possessed and which he could place alongside other definitions for comparison. But some words present special technical difficulties ; they seem to be presupposed in discussions of them ; and these are the words about which the majority remain Socratics. (It is a pity that philosophers should listen to that inner voice—it is only their mother tongue—which whispers intuitive and maybe inexpressible meanings, rather than co-operate to solve the technical difficulties. After all, these or similar difficulties occur also in cases—higher mathematics— where such philosophers are presumably less willing to think of the meanings as either intuitive or inexpressible. . . . However, it is sometimes called " the philosophy of common sense ".)

Let us take a concrete instance of Naaman's fallacy. Professor Ayer has claimed, in an argument which Professor Price has also accepted,[1] to demonstrate the *logical* primacy of the sense datum language. He states what he considers the necessary conditions of something being a physical object : its appearance must obey certain kinds of order—but these, he says, probably rightly, are contingent facts ; there could be a world in which only different kinds of order were exemplified and in which therefore there would be no physical objects. This looks plausible. But again

[1] A. J. Ayer, " The Foundations of Empirical Knowledge " (1940), § 23, " The Terminology of Sense Data," § 6, in MIND, liv (1945) ; H. H. Price in MIND, l (1941), pp. 292-293.

the standpoint is that of someone who begs the question by possessing both words for physical objects and words for sense data. What are the things (appearances) which have these kinds of order ? Ayer calls them sense data, and of course if one starts with sense data one is left with them at the end.

The logical analysis is this. His account of the conditions under which an object is called " physical " is descriptive, not simply conventional—why *we*, in Hume's words which he quotes, " attribute a continued existence to objects ". But some other persons than " we " may not attribute it in the same circumstances. Thus we might say that Ayer's conditions are sufficient but not necessary, and his conclusion suffers formally from the fallacy of denying the antecedent.

It is suggested, as we have just seen, that there might be objects possessing such unusual or scanty types of order that they could not be described as physical objects. To do such a view justice I think we must take it to mean that the objects would not be describable by a quantitative language, by a science which used measurement. But this surely implies an unacceptably narrow definition of " measurement ". Numbers may be assigned to frequencies of given light waves (quantities) by reference to a standard metre, and numbers may be assigned to given colours (qualities) by reference to a standard colour chart ; the only difference in principle between the two sorts of measurement which (owing to degrees of resemblance) is really ordering in series, seems to me to be that the quantities form a continuous series and the qualities a discrete series. To present-day science this distinction should be irrelevant. For in the first place there are easy mathematical devices for treating discrete sets of values as though they were continua—witness the everyday practice of statisticians ; in the second place it has turned out as a fact that certain fundamental physical quantities, such as electrical charge, *are* discrete.[1]

Also we must distinguish between " not being describable by scientific terms " and " not being interpretable in scientific, *i.e.*, general laws ".

The Physical Object Language

It remains to point out that, so far from there being some uniqueness about the sense datum language, the physical object language is an alternative already in use by physics and one which

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[1] Strictly, a physical object language does not designate colours etc. as private introspectible objects, but as types of behaviour.

is fairly easily to be extended to other sciences. I propose only to make a few general remarks about it : but these may suggest ways of meeting some of the remaining possible criticisms. Let me say again that I am not claiming *philosophical* preferability for this language any more than I have been allowing adherents of a sense datum language to claim that for theirs. Nor, again, does it entail any metaphysical commitment. To the question whether conscious states like anger *are* states of physical objects it gives no answer ; but it will describe them as such. An assertion by James that he sees bright flashes in front of his eyes may theoretically be checked by Peter if he observes a galvanometer attached to the visual area of James's cortex. They are not observing the same area of space, but they are observing overlapping bits of it. Most students of philosophy are taught to object : " You are forgetting that our knowledge of the existence of the cortex and visual areas comes from sensations ". This is true but irrelevant. It was answered, I believe, when it was suggested that the argument for the epistemological priority of a sense datum language started from a fact, but a fact which did not warrant the conclusion. Observation-statements neither state nor presuppose (imply) that they are observation-statements. Neither James's nor Peter's reports will mention the historical processes of observation. We *have* sensations, if you like, but we do not observe them. They may be mentioned in a third report, but this report will not mention its own genesis. One may pile report on report, but he will never catch one on two levels at once.

Those who adopt the point of view from which this article is written often enough make questions of philosophical theory or choice of language out of apparently factual questions. But the opposite process is sometimes more promising. For philosophical puzzles may logically be about words, but they are abstractions from *puzzlements*, and these have their roots in facts. For example, one of the puzzling facts of psychology is the obstinacy with which people apparently refuse to have the sense data that we should expect from a study of the sense organs. They persist in seeing the physical shapes and sizes and colours of things instead of the projected or perspective qualities. The common jay is believed to have the same disability. It seems possible moreover to substitute for " sense data " those characters of things which generally obey the so-called Constancy Hypothesis, *i.e.*, are picked out according to their projective properties, and for " physical things " those characters which do not follow the Constancy Hypothesis because the organism adjusts itself to them.

This would enable us to deal objectively, *i.e.*, in a physical object language, with whatever facts underlie the sense datum theory. Sense data will not, however, be entities but functions of an organism and its environment.[1]

The same kind of approach enables us to meet that suspicion of circularity which still hangs over our " physicalist " theory. We referred to the irrelevance of " the historical processes of observation ". Now irrelevant they may be in that context (it might be objected), but they must be describable. True, and the important thing for us is to see that, if our theory of knowledge postulates a scientific language, we are not merely entitled, but committed to a scientific account of knowledge itself. The philosopher's analysis of knowledge is no more than the naming of correctors : anything more is the task of the scientist. But then a causal theory of perception is commonly held to have been often refuted. To this we retort that the refutations must assume a sense datum language. When Ayer, for example, calls the theory " logically untenable " all he means to do is to object to a causal argument from sense data to things which are not sense data.[2] If there is to be objection, however, to the sort of causal theory that we should hold it must be like this. The theory makes knowledge depend on observation, and observation depends on the not wholly reliable central nervous system : but our knowledge, not just of the existence, *but of the degree of reliability* of the nervous system, depends itself on observation. To this, I think, the answer should be : True. But you must distinguish what may be called scientific circularity from philosophic circularity. A philosophical theory of knowledge which tried to " explain " knowledge in terms of non-scientific concepts would defeat its own purpose if its definitions were circular, *i.e.*, committed a *hysteron proteron*. But in our view it is the nature of a theory of knowledge to indicate correctors, that is, to state the rules of a language. Such a theory could not be circular, since it professes to explain nothing. That is why, when philosophers press us for our " theory " of perception, meaning our *explanation* of it or the validity of it, we give it in the only possible way—as a scientific, that is, causal theory. The sense datum language embraced the scientific language as a part of itself : in our case we have decided that the scientific language can be made

consistent without outside help ; the total language is thus co-incident with the language of science. Now in a scientific language—to be distinguished of course from a meta-language of science—apparent circularity is not a reproach, but a badge of honour. It is the mark of an advanced, or quantitative, science. The physicist's measurement of temperature depends on the measurement of length, which depends on the measurement of temperature. But by means of the method of successive approximations he avoids an impasse. (*E.g.*, he corrects his thermometer by his barometer, his barometer by his corrected thermometer, his corrected thermometer by his corrected barometer, and so to any degree of accuracy.) So we say that we correct the evidence of our eyes by the evidence of the photographic plate and the evidence of the photographic plate by the evidence of our eyes. There is no circularity provided that we attach no meaning to certainty except as a fictitious limit of probabilities. And this is the way of science, although it is not the way of philosophy, in which only necessary statements are made.

V.—IS THERE A SENSE OF DURATION?

By B. Mayo.

EDDINGTON once drew a distinction between our apprehensions of "time-extension" and of "space-extension".[1] "When I close my eyes and retreat into my inner mind, I feel myself *enduring*, I do not feel myself *extensive*". He concluded that our acquaintance with time (in the sense of "duration") is so intimate that, unlike space, it defies analysis. Now, when I "feel myself enduring", what is it that I "feel"? If, as Eddington implied, the whole of our experience, including experience which does not involve the external sense-organs (but excluding, perhaps, mystical experiences and the like) contains this "feeling of enduring", it would be a sort of un-analysable residuum. It would be indescribable, for the same reason that "the whole of experience" is indescribable; it would be common to all experience, and to describe it would be to apply to it terms appropriate only to specific types of experience. So it would not be surprising if the question, "What do I feel when I feel myself enduring?" should not admit of a satisfactory answer.

But there is, perhaps, this to be said about it. We are all certainly familiar with the experience of estimating how long ago a certain remembered event occurred, or how long an interval separated two such events. This seems to imply that we carry out some sort of measuring process. What this process is I shall discuss later. Here I should like to ask, what is measured? We might say, "What is measured is duration". Thus there are two meanings of "duration": one equivalent to "enduring" which we "feel", and one equivalent to "length of time" which we "measure". But since it is, presumably, from this single type of experience that the word "duration" gets these meanings, the conclusion would seem to be that the sense of duration and the faculty of measuring length of time are the same thing.

Now we measure lengths of things in space by comparing them with objects of standard length. Similarly we measure intervals of time by comparing them with intervals of standard length. Both processes involve sense-perception or its analogue

[1] *Nature of the Physical World*, ch. III.

of the " specious present ".   We decide by visual means whether
the extremities to a given object coincide with, fall short of, or
exceed those of the standard object, such as a foot-rule.   Similarly
the awareness of simultaneity and of ' before ' and ' after '
(the two cases of non-simultaneity) occurs within the experience
of the " specious present ".   But here, with the end of the actual
process of correlating, the similarity ends.   The spatial object,
once measured, remains measured ; it is easy to have an image
of the object together with an image of the " measurement ", such
as a number of hands or a yardstick.   But there can be no image
of the interval measured or of the interval by which it was
measured.   All that can be revisualised are the events marking
the beginning and end of the interval.   It is as if the spatial
object could not be imaged, but only its extremities.   This state
of affairs is connected with the difference between a perceived
object and " felt " duration : the one is apprehended as a whole
and is remembered as such, the other is not.

We can look at an object without measuring it ; but there is a
sense in which we cannot endure through an interval of time
without measuring it : for, as I have suggested, to endure *is* to
measure.   Since there does not seem to be any literal measuring,
what is the justification for using the word " measure " at all ?
It is that some of the essential elements of the measuring process
are retained.   These are (*a*) comparing with a standard and (*b*)
" knowing how much ".

Taking (*b*) first, we nearly always know, roughly, how much
time has elapsed since a certain remembered event.   This point,
obvious enough it itself, is also emphasised by the fact that
words referring to different lengths of time, flexible but only to a
limited degree, are in common use : words like ' long ', ' short ',
' momentary ', ' interminable ' : and the fact that most of these
terms are relative implies some kind of comparison.

The difficulty arises in considering (*a*) : what is the standard
we use ?   What is it that offers a basis of comparison by which
we estimate lengths of time ?

The first thing to notice is that the expression " a length of
time " is a purely metaphorical one.   It is not the least like " a
length of string ".   There is no substance, Time, which can be cut
up into bits.   The point would hardly be worth making, except
as a warning against the metaphysical implications of common
language : for in everyday life we do talk about measuring time
in language appropriate to measuring bits of string and the like.
All that there is is events ; the intervals between them are,
strictly speaking, nothing.   The " length of time " occupied by,

say, a race is really a nonentity ; the race consists of a series of events, two of which are specially important for practical purposes, the events constituting the beginning and the end of the race. Such events are, again for practical purposes, considered as instantaneous. Now it is a fact that we do estimate intervals between such events. And it will not do to say that we estimate these intervals in the same way as we estimate spatial lengths, when, for example, we try to imagine how many successive applications of a foot-rule would be needed, as if we are carrying out the actual mechanical measuring process. There are two reasons why this would be unsatisfactory.

(1) Dingle has pointed out [1] that there is no mechanical process for measuring time. What is measured, when we think we are measuring time, is actually space. The process known as " measuring time " consists of two parts : firstly, the correlation, in the relation of simultaneity, of two pairs of events, one member of each pair being, let us say, the position of the hand of a clock. The rest of the process consists entirely of spatial measurements. (These may be of different kinds : for instance, radial, as with a clock ; or volumetric, as with an hour-glass ; or linear, as (conceivably) with the descending weight of a grandfather clock).

(2) It may be objected that we do not need to imagine a process of measurement in order to estimate spatial lengths : it may be sufficient to estimate that this stick is longer than that and shorter than a third. In the same way, it may be said, we can estimate lengths of time without thinking of clocks ; we can estimate that one period is longer than another. But, in the case of the sticks, we still have to carry out an imaginary correlation : we think what it would be like for the sticks to be placed side by side with ends adjacent. Is there any analogous imaginary correlation in the case of the time-intervals ? If there is, it would have to be held that whenever we compare two time-intervals we visualise them as sets of successive events, imagining the initial events as if they were simultaneous and trying to " see " which of the two terminal events would occur first. I do not think that anyone would wish to hold such a view. I shall give some reasons at a later stage. Here I shall mention a further reason. The view does not appear to be in accordance with experience. It does not seem to me that estimates of duration depend entirely on the relativity of the time-interval between the events in question to other possible time-intervals—to time-intervals, that is, between similar pairs of events (a qualification

[1] *Through Science to Philosophy*, ch. XI.

which I hope to make clear in the next paragraph) whether these
other time-intervals are measured or not.   There seems to be a
definite ' quality ' about some remembered time-intervals which
cannot be easily reduced without residue to qualities of the events
in them : something that enables us to say without hesitation that
they are longer or shorter than others.   Yet how can intervals
have qualities in any sense whatever ?

We have the paradox, then, that we do estimate intervals
between events ; that this estimation implies some process, if not
of measurement, at least of comparison ; and that no such
process is to be found in the experience of " duration ".   This
paradox can, I think, be resolved only on one condition.   This is,
that in spite of appearances to the contrary, there are events
occurring in a continuous sequence and at a rate taken as uniform
and standard, with which the original events are correlated. Let us
assume, for the sake of argument, that there is such a sequence of
events, and that events apprehended in the ordinary way are
correlated with them in some manner.   Then we may suggest that
if a certain number of these postulated events have occurred
between the two original events, a certain definite impression of
time-lapse will ensue.   Furthermore, the fact that these events
have been occurring, are occurring, and are going to continue to
occur, must give rise to the feeling described as the sense of dura-
tion.   These events whatever they are, are not apprehended as
events ; consequently the "intervals" between them are not in
need of a second measuring-scale, so no vicious regress is involved.
Similarly, when I suggested above that these events occur " at a
rate taken as uniform and standard ", the term " rate " does not
involve circularity.   At least, it does so in no greater degree than
does the word " length " : no one would wish to argue that we
cannot know what it means to say that a stick is four feet long
unless we know how long a foot is.   The chief difference between
the two cases is that the length of a foot is conventional whereas
the rate of occurrence of the postulated events is not.

What is the point of introducing these hypothetical events ?
I shall suggest that they may not be hypothetical.   They may,
for instance, be connected with the physiological processes of the
individual's body.   There is a certain amount of evidence that
there is more than one such sequence of events, physiological or
otherwise, and, further, that they differ greatly in accuracy.
(" Accuracy ", of course, properly applies to the results, that is,
the estimates as compared with a clock or calendar).   I shall take
the most interesting case first.   Gunn has drawn attention [1]

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[1] *Problem of Time*, p. 386.

to the well-authenticated cases of a supernormal " duration-sensitivity " manifested during hypnosis.   In several instances the patient, while under hypnosis, was ordered by the hypnotist to perform a certain action after a given number of seconds (often amounting to several hours or even days).   He actually performed the action, which might be quite a complex and even extraordinary one (pulling a certain person's nose is one instance quoted), and the performance occurred exactly at the expiration of the prescribed number of seconds.   Several of the patients, on being asked how they knew when the time had arrived, said that they had an image of a clock pointing to a certain hour.   Often the prescribed action took place after recovery from the hypnosis.   This evidence would seem to argue an astonishingly precise " sense of duration " in the subconscious ; it is not yet explained by psychologists.

Under normal circumstances, of course, the " sense of duration " is anything but precise and is liable to wide divergences when checked by other means.   This indicates that the postulated sequence of events is itself irregular, just as a tape measure which gave results at variance with those obtained from a rigid measuring rod would normally be assumed to be stretched in places.   An example of an irregular sequence of similar events would be the series of heartbeats.   Of course, I am not suggesting that this particular sequence of physiological events, the series of heartbeats, bears any close relation to the " sense of duration ".[1] It it did, we should expect time to pass much more slowly after a period of violent physical exertion.   But there are other rhythmic bodily processes which might conceivably be related to the " sense of duration ".

These would account for the " sense of duration " as experienced in complete isolation—in Eddington's example, for instance.   But in ordinary life I think there is yet a third sequence of events in relation to which duration is measured.   This is the sequence of actual physical events presented by the external world to the observers, or else brought about by his actions.   Such events are of two types.   One is the series of more or less fixed period-markers, usually connected with physiological processes.   Examples of this type are mealtimes and bedtime.   These events constitute the framework of normal daily life ; other activities are correlated with them, so that we " know " whether a certain job can be done, say, " between tea and dinner ".   There are also the more public period-markers such as sunset, " blackout ", works sirens, curfew, or the Athenian " crowded market "

[1] Though Russell (*Analysis of Matter*, p. 363) has asserted that it does.

and " lamp-lighting ". Events of the second type are those constituted by individual activities. Even when disorganisation of normal life has upset the routine time-markers, it is still possible to form some impression of the lapse of time between events, although it will be a less reliable one. A day in which you do very little seems long in transit, yet short in retrospect ; and the converse is true of a day crowded with activity.

At this point we need to examine rather more closely the expressions " seems long ", " seems short ". What exactly do we mean when we say that the day " seems long " ? It is obvious that the apparent length of the day is not a sensible quality : yet it must be reducible to some fact or facts of experience. These will be, I think, facts such as the experience of boredom (the German *Langweile* expresses the connexion very well) and boredom in turn is probably reducible to certain yet more primitive experiences such as ' wanting to act ' combined with ' being frustrated from action '. Similarly the day " seems short " in proportion as these experiences are absent. I do not pretend that this is anything like a complete analysis of the situation. There is also, for instance, an element of surprise, as for example when we look at our watch and find that it is seven o'clock instead of half past five.

But when we say that the day " seemed long " or " short ", the change of tense marks a radical change of situation. In the first case (discussed in the last paragraph) we are not really estimating duration at all. We are not concerned with time-intervals separating events, but with the kind of experience we describe in such terms as " time passes slowly ". (This seems to imply that there is not only a time T 1 which can be used to measure the rate at which physical events occur, or at which bodies move, but also a time T 2 which measures the rate at which time T 1 moves. This apparent implication, on which Dunne's theory of Serialism is based, is of course an illusion arising from a metaphorical use of terms denoting movement in order to describe such psychological states as boredom.) In the first case, then, the apparent length of the day in transit is not an estimate of a time-interval, but an aspect of certain physiological or psychological states. In the second case, the apparent length of the day in retrospect is an apparent time-interval and its value depends on the number of events (in the ordinary physical sense) which occurred in it. Thus the same day can seem long in the first sense and short in the second.

Nor is this situation peculiar to the estimation of days. There is a very attractive theory which explains the difference in the

apparent length of years.   The years seem progressively shorter as we grow older ; and this is because the number of events in each year, compared with the total number of events in that and the preceding years, yields a fraction which becomes progressively smaller.   If a man has lived x years, and there occur (for the sake of simplicity, let us say) y events in each year, then the total number of events in his lifetime up to that point is xy, and the proportion of the events in that year to those in his lifetime is

$\frac{y}{xy}$ or $\frac{1}{x}$.   As x increases, this fraction, which on this view re-presents the apparent length of the year, decreases.   This would explain why, in spite of the fact that things seem to go on quite normally from day to day, nevertheless in retrospect the year seems shorter than the years of an earlier age.   This, of course, would not be the whole truth.   It would not explain, for example, why the apparent decrease is, with most people, much more marked in the period immediately succeeding adolescence.   This, however, might be accounted for by suggesting that different events have not all the same weight in the computation ; the events of adolescence, even the commonest, do seem to acquire a heightened interest and importance with the approach of maturity and this additional intensity quickly fades.   This fading might explain the acceleration of the apparent shortening process.

We are now in a position to elucidate further the almost paradoxical situation involved in the " measurement " of time.   Take any four successive events, A, B, C, and D. Assume that A has occurred and B is just occurring.   As B occurs, I form an impression of the time that has elapsed between the occurrences ; or, to put it less equivocally, an impression of the interval between A and B.   Assume the same process carried out with C and D.   I am now asked to say which of the two intervals is the longer.   At first sight this seems perfectly simple ; it is the sort of thing we are always doing.   But, at second sight, it seems quite impossible.   How can it be done ? It might be said that I compare the impression formed at the occurrence of D with that formed at the occurrence of B, just as we can say (within limits) whether a given piece of wood is longer or shorter than another one which does not happen to be present but which I can remember.   I then compare the given piece of wood with the memory-image of the other.   But how, it may be asked, can I compare the present impression of duration with the past one ?   When remembering the past impression, what exactly is it that I remember ?   I don't remember the

"impression" in any intelligible sense : I don't remember the impression of the piece of wood, but the piece of wood. Then what is it that I remember ?

Perhaps what I remember is just A and B, the events. But I must also remember C ; and, since I actually compare the intervals after D has occurred, I remember D as well. Now if all I remember is A and B and C and D, how can I also remember enough to enable me to say which of the intervals A-B, C-D is the longer ? Qualities and relations will not help. Qualities of the events are clearly insufficient ; and even relations between the events will not be enough, because these can only be the relations of ' before ' and ' after ', which have no reference to duration.

But the difficulty vanishes if we postulate a series of events, independent of A, B, C, and D, with which A, B, C, and D were themselves correlated at the time of their occurrence. If present duration is measured by the number of these events occurring in the present, then past duration will be measured by the number which occurred between the physical events in question. Our postulated sequence of events must be considered as a permanent and perpetually extending time-scale, in relation to which all events as they occur are located once and for all. This means not only that it will be possible (as, of course, it actually is) to say how long ago the event A, or the event B, occurred ; it will also be possible (as, again, it actually is) to say roughly how much time elapsed between A and B, and between C and D, since there is a fixed number of events between them. Thus it is possible to compare the intervals between two pairs of events and to say which of them is the longer.

I am aware that much of this will appear highly controversial. I have maintained that certain facts connected with memory and with the use of the measurement language as applied to time cannot be explained except on two major assumptions : firstly, a process or correlation, and, secondly, a standard or standards. Either of these assumptions may be questioned. The second led me to postulate a sequence of events constituting the required time scale ; and I have suggested certain directions in which such sequences might be sought.

VI.—DISCUSSIONS

MR. BAYLIS ON "FACTS"

I WANT to point out certain difficulties involved in the opening
pages of Mr. C. A. Baylis' paper on "Facts, Propositions, Exempli-
fication, and Truth" (MIND, N.S., LVII, No. 228). These diffi-
culties seem to me to vitiate much of what he has to say and
constitute my chief objection to his principal thesis. This thesis
is summed up by saying that "the relation between a true proposi-
tion and any fact in virtue of which it is true is . . . one of
characterisation and the converse relation one of exemplification".
In setting out his preliminary arguments for this thesis, he appears
to distinguish four different elements. These are (1) entities or
particulars ; (2) facts ; (3) propositions ; (4) statements. Putting
forward a theory of truth, Mr. Baylis is naturally interested in
defining the relation between (2) and (3), but his definition rests
more or less explicitly on definitions of the other relations. The
three relations can be stated by saying that (2), "at the lowest
level" at least, are relations between the elements denoted by (1).
("Facts are entities in relation" or "relations among particulars" ;
or relations of exemplification between particulars and characters
which characterise them.) (3) are related to (2) in the relation of
"characterising" ; or, conversely, (2) are related to (3) in the
relation of "exemplifying". (4) "express" (3).

Mr. Baylis claims to prove (p. 461) that there are facts as well
as entities or particulars ; and it is just this passage that I find
difficult to follow. To begin with, it is not clear, even if we leave
aside the invulnerably vague "entity", what is meant by a
"particular". Three consecutive examples of a particular in
this passage are a red rose, a sense-datum, and a building. That
the rose is red, that the sense-datum (a red patch) is red, that this
building is higher than that, are examples of facts. Of course
the introduction of the sense-datum terminology begs important
questions. I don't see, for example, how it can be maintained
that the statement "the sense-datum is red" is analogous to the
statement "The rose is red", or even that it makes sense. We
can hardly dismiss the term "sense-datum" as an unimportant
red herring, because it is just the element of "immediate experience"
or "direct awareness" associated with "sense-data" in contrast
with objects that Mr. Baylis is discussing when he says that we
have the same kind of evidence for facts as we do for particulars.
In applying the term "particular" indifferently to sense-data
and to objects, Mr. Baylis is trying to make it cover too much.
You can't hope to combine in a single category the subjectivity
of "direct awareness" and the objectivity of the public world.

Sense-data and buildings require different treatment. Indeed, putting them in the same class (" particulars ") seems to me to defeat the purpose of the " sense-datum " terminology, which was to define a class of " entities " which were not objects.

If this difficulty is not immediately obvious, it is only because Mr. Baylis has chosen the standard example of a " sense-datum " —the coloured patch. It has been the misfortune of this particular piece of philosophical equipment to have a foot in two worlds. When you are talking about " immediate experience " you want to mean by " coloured patch " a certain area of the visual field considered as completely unanalysed and undifferentiated except by colour boundaries. When you are talking on the commonsense level you mean something like a piece of material sewn on to somebody's coat. In the second case, of course, the relation between the sizes of two patches is certainly analogous to the relation between the sizes of two buildings. In the first case it's quite different. Indeed, there's no such relation, because the relation between " sizes " is a relation between objects, not sense-data.

My next point is that, even if we confine ourselves to objects, and mean by " patches " material patches, Mr. Baylis has still not convinced me that there are facts as well as particulars. I will deal first with the statement " We cannot avoid admitting facts about particulars as well as particulars, because to describe a particular by means of an adjective is simply one way of saying that the particular has, in the sense of exemplifies, the character signified by that adjective ". Now taking Mr. Baylis' example of the rose, the assertion is that we can't avoid admitting facts about the rose as well as the rose, because to describe the rose as red is simply one way of saying that the rose has the character signified by the adjective " red ". And earlier in the page Mr. Baylis asks ·" Will it not suffice to admit a red rose without agreeing that there is a fact of the rose's being red ? The answer appears to be ' No '." A red rose, then, he is prepared to regard as a particular. But, in that case, the question doesn't arise. It isn't a fact about this particular—about this *red* rose's being red—that exemplifies the proposition " the rose is red ". It's a fact about the particular, this rose. If the proposition were " the red rose is fragrant ", the corresponding fact would indeed be one about the particular, this red rose. This seems to imply that the kind of thing referred to as a particular varies according to what you are going to say about it. But this won't do, because it has to be something unvarying, as is implied by the term " substance " where Mr. Baylis gives " particular substance " as an alternative for " particular ".

Mr. Baylis' main argument for the existence of facts is that " we have the same kind of evidence in immediate experience for facts that we do for particulars, namely direct awareness ". His first example about the difference in size between two sense-data is open to the objection I have already considered. I shall confine

myself to the second—awareness of the redness of a patch—where I shall assume for the moment that the argument would be unaffected by interpreting " patch " as " material patch ". Mr. Baylis says " Observation of this fact " (the fact that the patch has the character of being red) " is our evidence and our only evidence for asserting that this particular patch is a red patch ". Here he seems to be adding another relation to the list, a relation between (2) and (4). Facts exemplify propositions ((2) and (3)) ; they also, when observed, constitute evidence for statements ((2) and (4)). Observation of the fact that a patch is red (or " has the character of being red ") is " evidence " for the statement that the patch is a red patch. Here I wish to ask what purpose is served by bringing in at this stage the notion of " evidence ". If I state that a patch I am observing is red, I require no evidence. I state what I observe, not something for which what I observe constitutes evidence. Or if I do require evidence, it can only be because I doubt whether I am observing correctly : I doubt whether the patch really is red. But then the evidence I require will be other people's statements, or readings from an apparatus for measuring wavelengths. My own observation will not be evidence, because it is now *ex hypothesi* ruled out.

Perhaps after all the assumption that " patch " meant " material patch " did affect the argument. Let us assume the contrary. I can't easily visualise an immaterial red patch ; but it's a matter of experience that if I close my eyes in bright sunlight I " see red " without seeing any material object. This time I can't even look for external evidence to confirm that what I am seeing really is red. But, again, I require no evidence for the statement that I am seeing red. Mr. Baylis says that my observation is my ground for believing that a red patch is part of the content of my experience. This statement amounts practically to a *reductio ad absurdum* of the view that facts are objects of " direct awareness ". For what would it be like to disbelieve that a red patch were part of the content of my experience ? Even an hallucinatory red patch is at least that. It's very odd to suggest that what I'm actually observing mightn't after all be part of the content of my experience.

B. MAYO.

6

A NOTE ON ROUSSEAU, *CONTRAT SOCIAL*,
BOOK II, CHAPTER 3

Did Rousseau, in " Le Contrat Social ", assert the infallibility of
the General Will ?

In ii. 3 we read : " Il s'ensuit de ce qui précède que la volonté
générale est toujours droite et tend toujours à l'utilité publique ;
mais il ne s'ensuit pas que les délibérations du peuple aient toujours
la même rectitude. On veut toujours son bien, mais on ne le voit
pas toujours ; jamais on ne corrompt le peuple, mais souvent
on le trompe, et c'est alors seulement qu'il paraît vouloir ce qui
est mal."

Three of the four English editions[1] of the *Contrat* in Bodley
translate the second line of the quotation as " the general will is
always right ", while the 1764 edition prefers " in the right ".
Three editions add : ' but it does not follow that the deliberations
(or resolutions) of the people have always the same rectitude '.
Professor Cole, perhaps more consistent than the others, prefers
the rendering : ' but it does not follow that the deliberations of
the people are always equally correct '.

The point at issue is, as the title of ii. 3 indicates, ' si la volonté
générale peut errer '. The rendering ' the general will is always
right ', makes it appear that the question is already settled, as if
Rousseau's hypothesis was purely academic. That this is not
the case will be shown later ; Rousseau cannot be reduced to such
clear terms at this early stage. Although the people desire their
own good, he declares, they don't always see it, and they are often
deceived. In at least one other passage of the *Contrat, viz.* ii. 6,
the same distinction is repeated : ' La volonté générale est toujours
droite mais le jugement qui la guide n'est pas toujours éclairé '.
In any normal political assembly, a vote taken under a misappre-
hension or through wrong guidance could hardly be right, but ac-
cording to the translations, the general will, overcoming ignorance
and possible misdirection, by some process of its own, is ' always
right '. This is a mystery, a question of faith. Credo quia ab-
surdum.

It is true that at the end of ii. 3 Rousseau thinks to ensure
' l'énoncé de la volonté générale ' either by forbidding all groups
and associations or, by seeing that these groups and associations,

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[1] (1) The anonymous edition of 1764 printed for T. Becket and P. A. de
Hondt in the Strand. (2) The translation by Rose M. Harrington, Knicker-
bocker Press, 1893. (3) The translation by Henry Tozer, Swan Sonnen-
schein, New York, 1895. (4) The translation by G. D. H. Cole, Everyman's
Library.

if permitted, should be numerous and of equal power so as to cancel one another out. The citizen should preferably be alone *vis-à-vis* the State. ' Ces précautions ', he says, ' sont les seules bonnes pour que la volonté générale soit toujours éclairée, et que le peuple ne se trompe point '.

Does this mean that Rousseau has now discovered the only good precaution for enlightening the general will and saving the people from error ? Not at all. ii. 7 takes a much less sanguine view of the problem of legislation. In addition it would be easy for anyone with less *a priori* assurance and greater political experience than Rousseau to demonstrate that the ban on groups and parties could as easily corrupt as enlighten the general will.

But did Rousseau himself claim that ' the general will is always right ' ? This is most improbable, for the following reasons :

1. The adjective *droit*, except in phrases like *angle droit, main droite*, etc., where it qualifies concrete words, does not mean *right* in the sense that a decision may be called ' right '. Applied to abstract words it means rather upright, righteous, morally straight. In the paragraph quoted, Rousseau's sense would then become : ' the general will is always upright, *i.e.* it wishes to do the right thing, but it does not always succeed in attaining its ideal '. By admitting that the people are often deceived, he allows for that possibility of error which cannot be reconciled with the claim that the general will is always right.

2. It is obvious from what precedes ii. 3, according to Rousseau, ' que la volonté générale est toujours droite '. These chapters are not concerned with the question of infallibility, but with the predominantly moral nature of the social contract in which the purpose of the general will is ' le bien commun '.

3. If the general will is always right, why is it suspended in favour of dictatorship ' quand il s'agit du salut de la patrie ' (iv. 6) ? One should now have to amend the formula to : ' the general will is nearly always right ' ; or, if crises and wars were frequent, *nearly always* would become merely *sometimes*.

4. If in the chapters preceding ii. 3 it is obvious that the general will is always right, why in ii. 7 does Rousseau stress the importance of the hitherto unmentioned *législateur*? This superman, this demi-god, this paragon and oracle is introduced as guide and counsellor to the general will which has already been translated as always right. If he is necessary, the general will cannot be always right, and if it is always right, then the législateur is a superfluous encumbrance.

An attentive reader of the *Contrat Social* will be conscious of certain fluctuations in the author's moods. Sometimes he is serenely confident, at others full of misgivings. A temperamental man himself, he has only to recall his own experience and realise the difficulty of maintaining constancy of faith. It was in a mood of realistic doubting, perhaps after the daemonium meridianum had

been insidiously undermining his confidence, that he composed ii. 7. The complexity of the legislative problem has now dawned upon him. ' L'ouvrage de la législation ' he says is ' une entreprise au-dessus de la force humaine '.   And again, ' il y a mille sortes d'idées qu'il est impossible de traduire dans la langue du peuple '.   As Rousseau never claimed that only an intellectual and moral élite could qualify for the social contract, what then becomes of the idea of an infallible general will ?

Rousseau's meaning may be summed up as follows : the general will is based upon the moral principle that all private interests are subordinate to the overriding interests of the State ; the aim of the general will is ' le bien commun '.   Starting from this principle and reinforced by the other safeguards provided, such as the *législateur*, an enlightened general will has better chances of obtaining the best results than an opportunistic, capricious, or Micawberish policy of waiting on the day and/or hoping for the best.

This does not amount to a claim of infallibility, but in so far as morality is a constituent of judgment, the general will starts from a right principle and is directed to the right end.

This may be over-simplification of a difficult problem.   Few have been the object of more diverse interpretations than Rousseau, and for this Rousseau himself, by using ambiguous terms, is largely to blame.   As Professor Cole says in his introduction to Everyman's edition of the Contract : " It is impossible to acquit Rousseau, in some of the passages in which he treats of the general will, of something worse than obscurity—positive contradiction ".

Some at least of these obscurities and contradictions would disappear if translators discarded the belief that *droit*, applied to an abstraction, means ' right '.

F. A. TAYLOR.

MR. O'CONNOR'S "PRAGMATIC PARADOXES"

In MIND of July, 1948, Mr. D. J. O'Connor drew attention to four statements constituting what he called "pragmatic paradoxes". The peculiarity of these is that apparently, although they are not formally self-contradictory, they cannot conceivably be true in any circumstances : e.g., "I remember nothing at all" (where I must at least remember the proper use of the English sentence "I remember nothing at all"). In connexion with these paradoxes it is worth comparing some other statements with those mentioned by Mr. O'Connor. For instance, if I say "I remember something", apparently this statement cannot conceivably be false (for I must at least remember the proper use of the English sentence "I remember something"). But it does seem that I can intend it as a factual statement about my contemporary state of mind. Thus apparently on the one hand we seem to have statements which are not self-contradictory but cannot conceivably be true, and, on the other, statements which are not analytic but cannot conceivably be false. I wish to suggest that these paradoxes arise out of an ambiguity (not infrequently recognised) in the word "statement".

In one of the two senses of "statement" which I propose to distinguish it is an event-expression, like "motion", "laughter", or "physical training". Of anything which is called a "statement" in this sense it is legitimate to ask "When and where did (does, will) it happen ?". In another sense, however, "statement" is a logical expression, like "entailment", "non-contradiction", or "type fallacy". And of anything which is called a "statement" in this sense it is absurd to ask "When and where did (does, will) it happen ?". We can avoid the ambiguity by using "utterance" in the former sense, instead of "statement", and "proposition" in the latter. Thus utterances will, by definition, be events in space and time. Some utterances occur in speech, for instance, others in writing or in silent thought. And propositions will, by definition, not be events in space and time. I do not mean that propositions are subsistent entities, for I do not know what these are. But "utterance" and "proposition" stand to each other in such a relationship that it makes sense to ask, e.g., "When should I utter the proposition 'The cat is on the mat' ?", or "What proposition was his utterance intended to communicate ?".

Now there is nothing to debar propositions from describing utterances just as they describe other events. "The cat is on the mat", "All his utterances are in English", and "None of my utterances take place between 2 a.m. and 8 a.m.", can all be regarded as propositions. Accordingly there is nothing in principle to debar propositions from being such that they can be verified or falsified by their own utterance.

85

If I now utter the proposition " I remember nothing at all ", I should indeed be uttering a false proposition. But if I have my utterance recorded for a gramophone and the record is played at my funeral, the proposition uttered might then be true. Thus the proposition " I remember nothing at all " can conceivably be true. And the proposition " I remember something " can conceivably be false, if uttered in similar circumstances. But this can only be recognised if we distinguish " proposition " from " utterance " in such a way that the same proposition may be uttered in different circumstances at different times.

Similarly, the proposition " I am not speaking now " would be false if I spoke it aloud. But it would be true if I thought it silently to myself. And Mr. O'Connor himself mentions circumstances in which the proposition " I believe there are tigers in Mexico but there aren't any there at all " would be true : it would be true if I am lying when I utter " but there aren't any there at all ".

Mr. O'Connor notes that three of his four paradoxes resemble each other in being " statements in the first person which refer to the contemporary behaviour or state of mind of the speaker ". But he also mentions another paradox which does not contain any such egocentric particulars. " The military commander of a certain camp announces on a Saturday evening that during the following week there will be a ' Class A blackout '. The date and time of the exercise are not prescribed because a ' Class A blackout ' is defined in the announcement as an exercise which the participants cannot know is going to take place prior to 6 p.m. on the evening in which it occurs. It is easy to see that it follows from the announcement of this definition that the exercise cannot take place at all. . . . The conditions of the action are defined in such a way that their publication entails that the action can never be carried out." I suggest that, although this paradox differs from his others in not involving egocentric particulars, it resembles them in arising from a proposition that can be falsified by its own utterance. The proposition in this case is " A ' Class A blackout ' will take place during the following week ". This proposition is rendered false by its public announcement : it might be true if the camp commander told nobody of his intention to hold the exercise.[1]

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[1] If the camp commander intended to stage a surprise exercise on one day during the week and yet wanted to warn his troops of his intention, he would have to make an announcement somewhat like one or other of the following : Either " One day next week there will be a surprise exercise. A surprise exercise is an exercise about which, unless it takes place on the last day of the period for which you are warned, you will be in doubt as to when it is to happen until 6 p.m. on the evening in which it occurs." Or " One day next week there will be an exercise. Unless it takes place on Saturday you will be in doubt as to when it is to happen until 6 p.m. on the evening in which it occurs." In the former case he utters a prediction and a definition, in the latter two predictions. Owing

It would be interesting to know if there are any pragmatic paradoxes—statements which are apparently not self-contradictory but not conceivably true, or factual but not conceivably false—that do not arise from failing to distinguish between the "utterance" and "proposition" senses of "statement" (or similar words) when the proposition in question can be verified or falsified by its own utterance.

L. JONATHAN COHEN.

to the irreversibility of the time series, if it is known that an event will take place on either $t1$ or $t2$ or $t3$ or . . . tn, it will only occur as a surprise (in the ordinary sense of "surprise") if it happens on either $t1$ or $t2$ or $t3$ or . . . $tn - 1$. For this point I am indebted to a discussion with E. A. Gellner and K. Rankin.

" ETHICS WITHOUT PROPOSITIONS "

I WANT in what follows to comment upon two central points in Professor Barnes's stimulating Presidential Address to the 1948 Mind-Aristotelian Conference at Durham. The first is his contention that there is something queer or paradoxical about the notion of ' normative assertions '. The second is his admission that ' a peculiarly moral feeling ' must be recognised as a component of those ' attitudes ' which—according to his own theory—it is the function of ethical statements to ' express '.

I. ' NORMATIVE ASSERTIONS '

Barnes's criticism of ' normative assertions ' is presented with a brevity which is a little surprising in view of the weight which he apparently attaches to it. I do not doubt that he has other grounds for finding the traditional analysis of ethical statements objectionable, but as a matter of fact it is to this ground alone that appeal is made in his address. I give the relevant passage in full.

' Ethical statements are traditionally held to be normative assertions. But how can a statement both assert a fact and prescribe a norm ? There seems an obvious difference between telling someone that something is the case and telling him to do something ; between deciding that something is so and deciding to do something ; between stating that someone is acting in a certain way and recommending approval of his so acting.' [1]

I submit that this argument depends for any plausibility it may have upon a misleading suggestion as to the manner in which ethical statements are, on the traditional analysis, concerned with norms. If the traditionalist does suppose that an ethical statement *prescribes* a norm, admittedly he ought to be more embarrassed than he is by his inclination to say that it is also an assertion. But does he in fact suppose this ? Surely not. It is important to distinguish statements which *prescribe* norms (and are of the nature of imperatives) from statements which *affirm the existence of norms* (and are of the nature of assertions). If, as seems to me clear, ethical statements on the traditional analysis fall into the latter category, the alleged difficulty vanishes.

The distinction just noted tends to be blurred by the fact that, in non-ethical fields, the same form of words can be used for either function—prescribing a norm or affirming its existence. ' Candidates must write on one side of the paper only.' As spoken (or written) by the appropriate examination authority, this sentence

[1] *Arist. Soc. Supp.*, Vol. XXII, p. 2.

'prescribes a rule'. But suppose that (the instruction being given orally) one of the candidates fails to hear clearly what has been said, and asks a fellow-candidate if any rules have been laid down. He is told 'candidates must write on one side of the paper only'. As now uttered, the sentence does not prescribe, but states the existence of, a rule. In the former case the speaker is, and knows himself to be, the imponent of the rule : in the latter case the speaker is, and knows himself to be, not the imponent of the rule but a mere 'reporter' of the fact that the rule exists. Indeed, we might quite well point the difference between these two kinds of normative reference by using the very language which Barnes uses to distinguish between normative and assertive. In the former case the sentence 'tells him (the candidate) to do something', and in the latter case it is 'telling someone (the candidate) that something is the case' (*viz.*, that there is a rule that candidates must write on one side of the paper only).

It does not appear to me doubtful into which category ethical statements, on the traditional analysis, must be deemed to fall. 'You ought to pay back that money you borrowed.' A rule or norm is referred to, but the speaker does not conceive himself to be the imponent of it. If he did, he would not suppose himself to be making an *ethical* statement. He is asserting the existence of a rule which has application to a given situation.

I conclude, therefore, that whatever valid objections there may be to the traditional analysis of ethical statements, this analysis cannot be ruled out of court *ab initio* on the charge that the very notion of 'normative assertions' is formally absurd. I can understand, indeed, that many philosophers to-day may be predisposed to scepticism about the notion of 'normative assertions' by reason of a prior scepticism about what normative assertions, in the field of ethics at any rate, seem to assert ; *viz.*, the 'existence' of 'objective' moral laws. One's general philosophical principles may very well lead one to regard assertions which assert anything of this sort as extremely paradoxical. But in that case the 'paradox' will derive from the content, not from the form, of the assertion. Scepticism about 'objective moral laws' will not justify scepticism about normative assertions as such ; about, *e.g.*, the particular normative assertion that rules exist for the conduct of examinations.

But are ethical statements as a matter of fact normative assertions ? We shall be better able to give an answer to that question when we have considered what Barnes has to say on the second (and, I think, more interesting) topic of the 'peculiarly moral feeling'.

II. THE 'PECULIARLY MORAL FEELING'

Barnes's positive theory is, it will be recalled, that 'ethical statements express attitudes'. 'Attitudes', pro or contra, are

normally directed to *kinds* of action, and they have three components.

'If there is a kind of action which a man has a disposition to do himself, to encourage others to do, and to feel pleased at, when done by others, then he has a pro-attitude to that kind of action. An anti-attitude can be defined similarly.' [1]

But Barnes is clear that not all attitudes, so defined, are 'moral' attitudes. A man may, *e.g.*, have a favourable (or unfavourable) attitude towards smoking or drinking which is not what we recognise as a *moral* attitude.

'I think we must admit that if there is to be a moral attitude, it must contain at least one peculiarly ethical component, *viz.*, the pro- or anti-emotion which I feel when I contemplate actions of the kind in question.' [2]

It is in virtue of his recognition of this peculiarly ethical component, or 'peculiarly moral feeling' as he later calls it, that Barnes is able to claim that his ethical theory, although subjectivist, is not naturalistic.

On Barnes's view, this moral feeling is a recognisable constituent of our moral attitudes which distinguishes them from non-moral attitudes. Of its positive character as experienced, however, he has nothing to say. And it may be that there *is* nothing to say. It may be that it is a 'simple nature', ultimate and unanalysable ; definable, if definable at all, only by reference to the kind of actions to which it is most habitually directed. It may be, on the other hand, that it is not simple, but complex, and that its analysis yields consequences of capital importance for ethics. In any event, it is a matter to be settled not by assumption, but, in the last resort, by interrogation of one's own experience. We must evoke in imagination what seem to us unquestionably authentic representatives of moral and non-moral attitudes respectively, and see whether attentive scrutiny will not enable us to give some account of the difference between their feeling-components.

As an example of a non-moral attitude we may, with Barnes, take an attitude to smoking : though I shall prefer, unlike him, to consider the case where this is a favourable, rather than an unfavourable, attitude—if only for the reason that the votaries of smoking are less apt than its critics to invoke moral sanctions for their attitude. As an example of a moral attitude, that towards promise-keeping will do as well as another.

Suppose, then, we ask an ordinary unsophisticated person, who happens to have a moral pro-attitude towards keeping promises and a non-moral pro-attitude towards smoking, how he would describe the distinction between his specifically moral feeling towards the former and his non-moral feeling towards the latter. I suggest that he would by no means find himself at a loss for an answer. He

[1] *Arist. Soc. Supp.*, p. 19.   [2] *Ibid.*, p. 22.

would almost certainly describe the distinction in terms which, however objectionable in point of strict psychology, are yet surely not uninstructive. He would almost certainly say, I think, that towards promise-keeping, but not towards smoking, he has a feeling of ' oughtness ' or ' obligation ', a feeling that he and others ' ought ' so to behave.

Now so long as it is conceded (as by Barnes) that there *is* a peculiarly moral emotion, and so long as no rival account is offered of its nature as experienced, it seems to me that we are bound to take seriously the account which the plain man offers of it. His description, such as it is, holds the field until such time as it is shown to be untenable. It just will not do to say simply that the emotion is unanalysable, when we find that ordinary moral beings are perfectly ready with a description of it, and, what is more, with the *same* description of it.

As a matter of strict psychology, of course, the plain man's description *is* untenable. A feeling *that* . . . is not a mere ' feeling ' at all. But this technical offence does not invalidate, but rather throws into relief, the essential point of the description offered. What the plain man is trying to tell us when he says that his feeling is a feeling of ' oughtness ', a feeling that one ' ought ' so to behave, is, I think, that the so-called ' feeling ' has an aspect of *assertion*, or *judgement*, as well as of feeling ; that his moral feeling towards promise-keeping includes the *judgement* that promises ought to be kept.

The specific manner in which this judgement aspect is integrated within moral feeling clearly calls for further elucidation. A point has now been reached, however, at which it becomes reasonable to ask the reader whether, when he appeals to the testimony of his own introspection, he is really able to escape giving the same sort of answer as the plain man gives. If one is careful to prevent a preconceived theory about ethics—or perhaps about knowledge in general—from colouring one's report, can one really avoid recognising that what is distinctive about moral feeling is precisely that it is inseparably united with a judgement of ' oughtness ' ? If that is *not* its distinguishing mark, then what *is* its distinguishing mark ? And if it has no *describable* distinguishing mark, for the reason that what distinguishes it is some simple unanalysable quality, then what explanation is to be given of the apparent unanimity with which ordinary moral beings *do* offer a definite description of it ?

Let us, then, look more closely at the manner of integration of judgement with moral feeling. My contention is that the peculiarly moral feeling towards keeping promises involves the judgement that promises ought to be kept. The moral feeling, I am suggesting, is not *itself* without the judgement—it requires the judgement for the completion of its own nature. Two opposite, but equally ruinous, errors of analysis must be carefully avoided. We must not say that our moral feeling towards promise-keeping *generates*

the judgement that promises ought to be kept : for (we have urged) it is not the distinctively *moral* feeling at all unless the judgement of oughtness is already present. But neither must we say that the moral feeling is *generated by* the judgement of oughtness : for if this judgement is a moral judgement proper, and not the mere mechanical repetition of a conventional formula (if, in other words, the ' ought ' of the judgement is before our minds in its intrinsic moral meaning) then the distinctively moral feeling towards promise-keeping is already present. Most of the moralists who have held that the moral feeling is prior, and the moral judgement sequent upon it, have held this, I think, largely because it appeared to them that the only alternative was to suppose that the moral judgement *precedes* the moral feeling ; an alternative which they rightly reject. Similarly, most of the moralists who have held that the moral judgement is prior, and the moral feeling sequent, have held this largely because it appeared to them that the only alternative was to suppose that the moral feeling precedes the moral judgement ; an alternative which they, too, are right to reject. The possibility which is missed by both parties is that feeling and judgement are twin *aspects* of a *single* experience, neither of which has precedence over the other. This I believe to represent the true state of the case.

If this view is sound, then the Rationalist school and the Senti-mentalist school of moralists are equally right in what they affirm and equally wrong in what they deny. The ' peculiarly moral feeling ' shows itself on analysis to be something which may be called, indifferently, either a feeling with a judgement aspect, or a judgement with a feeling aspect. Neither can be excluded from the description of the essential moral apprehension. But it is quite crucial that the two should not be thought of as merely *conjoined*— ' judgement *plus* feeling '. The aspects are *aspects*, not *entities* ; analytically distinguishable, but only as abstractions from the unity of a single experience.

The ' double-aspect theory ' (as we may call it) of moral appre-hension is not, of course, something new in ethical thought. What else but this was in Bishop Butler's mind when he spoke of the moral faculty as alike ' a sentiment of the understanding ' and ' a percep-tion of the heart ' ?   In Butler, it must be admitted, such expressions are little more than *obiter dicta*, and they are not followed up either by himself or by his immediate successors. But there is exceedingly interesting evidence supplied by Hastings Rashdall that the double-aspect theory had authoritative currency in Oxford in the early years of this century. Rashdall quotes a clear instance from the letter of a friend who takes exception to his own account of the relationship between reason and feeling ; and in apologising for the publication of a private correspondence he observes that he ' cannot call to mind any printed expression of this doctrine, though it is taught by high authorities in Oxford '. The passage from the letter is worth transcribing in full :—

'I think that the "reason" and "feeling" which are to be found in moral judgements, though no doubt distinguishable, are not always found together, but each is unintelligible and empty apart from the other. The judgement "this is right" is not a moral judgement unless one has, more or less, the moral emotion (for in the judgement "this is right", when the ground is any authority, the moral emotion and the judgement proper fall upon the authority, not strictly upon the particular point), nor is it a moral emotion unless it claims universality. This, I think, is the same view as yours, but perhaps you might more carefully avoid the use of language which suggests juxtaposition (reason *plus* feeling); which is surely unsatisfactory, and leads to what one finds inadequate in the language of Hume on one side and Kant on another.' [1]

This is *not*, in fact, the same view as Rashdall's. Rashdall is fundamentally a Rationalist, as his reply immediately makes clear. But it *is* the double-aspect theory. Had the writer, or some other of the 'high authorities' alluded to, found an opportunity to develop the theory at length, and in print, it is not inconceivable that the later course of ethical thinking in this country might have been markedly different.

The conclusion to which we are forced is, I submit, this. It is only when the 'peculiarly moral feeling' is allowed to remain unanalysed that a subjectivist can at once recognise its existence and retain his subjectivism even a subjectivism on such relatively conservative lines as that so ingeniously developed by Professor Barnes. Barnes's suggestion is that the ethical statement ' X ought to be done ' can be interpreted as an expression of a moral pro-attitude towards X. But if a moral pro-attitude towards X admittedly contains ' a peculiarly moral feeling ' towards X, and if, as I have argued, this peculiarly moral feeling itself involves the assertion or judgement that X ought to be done, the interpretation is circular. The ostensible assertion with which we started, and of which we had hoped to rid ourselves by analysis, remains on our hands, as ' objective ' and as ' normative ' as ever it was.

C. A. CAMPBELL.

[1] Rashdall, *Theory of Good and Evil*, Vol. I, pp. 168-169.

VII.—CRITICAL NOTICE

Probability and Induction. By WM. KNEALE. Oxford : Clarendon
   Press. Pp. viii + 264.

THIS very able and interesting book is based on the lectures given
in Oxford by Mr. Kneale up to the outbreak of the second world war,
and has been prepared by him for the press in the scanty leisure which
he has enjoyed since that war changed from ' hot ' to ' cold '. It
forms an admirable general introduction to the philosophy of the
two inter-connected subjects named in its title, but what makes it
particularly exciting is certain special doctrines on fundamental
questions which Mr. Kneale asserts and defends. These are in con-
flict with certain philosophical principles or prejudices which are at
the moment fashionable and almost orthodox among Mr. Kneale's
contemporaries and juniors in this country and the United States.
These parts of the book are likely to lead to much valuable discussion.
It is a very happy circumstance that doctrines which are at the
moment unfashionable should be put forward by a man like Mr.
Kneale, who is fully aware of the strength and the weaknesses of the
current orthodoxy, and whom no-one in his senses can afford to
dismiss as a negligible ' back-number '.

The doctrines to which I refer are the following. Mr. Kneale dis-
tinguishes between matters of fact and what he calls ' Principles of
Modality '. He rejects the view that all statements which osten-
sibly record principles of modality are really statements about
language couched in a misleading form. He holds that, if there are
laws of nature, they are all principles of necessity, although none
of them can be known *a priori*. Lastly, he holds that what he
calls ' Probability Rules ', *i.e.*, propositions of the form ' The
probability of an instance of α being an instance of β is p ', are also
principles of modality, which cannot be known *a priori* but can be
reasonably conjectured inductively on the basis of statistics.
According to Mr. Kneale, the laws of logic, of phenomenology, and
of nature (which are all fundamentally of the same kind), leave open
a certain range of possibility for anything which is an instance of α,
and they leave open a certain narrower range of possibility for any-
thing which is an instance of $\alpha\beta$. What a probability rule asserts is
that the latter range bears a certain proportion to the former.

I shall begin by giving a rough general sketch of the contents of
the book as a whole, and shall then try to expound in greater detail
(so far as I understand them) these characteristic doctrines of Mr.
Kneale's and his reasons for them and against alternatives to them.

(I) GENERAL OUTLINE OF THE CONTENTS. The book is divided
into four Parts. The first, which is introductory, treats of *Knowledge*

and Belief. The second, entitled *The Traditional Problem of Induction*, is concerned with all the main philosophical problems of induction in so far as that process is used to establish *laws*, as distinct from probability-rules. The third, entitled *The Theory of Chances*, discusses the fundamental notions and theorems of the calculus of probability, and considers whether these are relevant to the logic of induction. The answer to the latter question is negative ; and the fourth Part, entitled *The Probability of Inductive Science*, deals with the question *whether*, and, if so, *in what sense*, recognized inductive procedures give more or less ' probability ' to statements of law and to probability-rules.

(1) *Knowledge and Belief.* Mr. Kneale's conclusions may be summarized as follows. He starts with the antithesis between ' knowing *p* ' and ' believing *p* '. He holds, in opposition to some distinguished epistemologists, that ' knowing ' is used in an occurrent sense, and not *only* in a dispositional sense. (*Cf., e.g.,* " When it began to pour with rain while I was out walking this afternoon I *knew* that I should get wet through ".) He has not met with any satisfactory analysis of ' knowing *p* ' in the occurrent sense, and so he takes it provisionally as unanalyzable. It is equivalent to ' noticing that *p* ' or ' realizing that *p* '.

The phrase ' believing *p* ' covers two different cases, which may be described as ' taking *p* for granted ' and ' opining *p* '. The former consists in acting as if one knew *p* when one does not know it. To say that *A* opines *p* with a certain degree of rational confidence means that (i) *A knows* certain other propositions *q*, which in fact probabilify *p* to the degree in question, and (ii) *A knows* that *q* probabilifies *p* to that degree. Opining may be irrational. This covers two cases. *A* may take for granted (instead of knowing) some or all of the evidence for *p* ; or he may take for granted (instead of knowing) that the evidence probabilifies *p* to the degree in question. Neither failure in rationality necessarily leads to false belief.

Probabilification of one proposition to a certain degree by certain other propositions is a purely objective fact. It has certain analogies to the necessitation of one proposition by another, and certain unlikenesses to it.

We talk of the probability of throwing a six with a fair die, and we also say that induction establishes certain laws and certain probability-rules with high probability. Mr. Kneale holds that the word probability is used in different senses in these two applications. But it is not just a single word with several totally disconnected meanings, like the word ' plot ', *e.g.* There are real and important analogies between its various applications. A most important common feature is that it is reasonable to take as a basis for action any proposition which is highly probable, in the appropriate sense, on the evidence available to one. Any satisfactory analysis of ' probability ' must enable us to see why this is so.

(2) *The Traditional Problem of Induction.* Taking induction for the present as a process by which universal propositions are established, Mr. Kneale points out that the word has been used to cover four different processes, each of which leads to a different kind of universal proposition. These processes may be described as *Summary, Intuitive, Mathematical,* and *Ampliative* induction.

Summary Induction establishes propositions of the form All S is P, where the description ' S ' is such that, from the nature of the case, it can apply only to a finite number of instances, and where it is in principle possible to know that one has exhausted the whole set. An example would be : All the chairs in this room on Christmas Day 1946 had red seats. Mr. Kneale points out that such a statement is equivalent to : No part of this room during the period in question was occupied by a chair with a seat which was not red. This is different in kind from such a proposition as : All men are mortal. Summary Induction is a deductive argument, though it cannot be reduced to a syllogism in the technical sense. One premiss has to be what might be called an ' exhaustive ' proposition ; *e.g.,* This, that, and the other sub-region together make up the space in this room.

The result of Intuitive Induction is knowledge of what Mr. Kneale calls ' Principles of Modality ', *i.e.,* of compatibility or incompatibility between characteristics. These are essentially universal and necessary. An example would be : No surface could be red and green all over at the same time, but a surface could be at once red and hot all over. It is a characteristic doctrine of Mr. Kneale's that such propositions are *not* merely linguistic. His arguments on this point will be considered later. We may sum up Mr. Kneale's account of intuitive induction by saying that he considers it to be a valid intellectual process, but not a form of reasoning. What experience does here is to provide *instances,* not premisses.

Mathematical Induction, or argument by recurrence, establishes propositions of the form : All numbers have the property *p.* Such propositions are necessary, but they differ in kind from principles of modality which are established by intuitive induction. After considering various alternative views as to the nature of propositions about all numbers, Mr. Kneale puts forward the following account of them. To say, *e.g.,* that 2 is a number, is to say that ' 2 ' is a *recurrence symbol, i.e.,* that it signifies, not an individual nor a character of an individual or a group, but a certain feature, *viz.,* a recurrence in the structure of such facts as are expressed by sentences like ' There are 2 tables in this room '. To say that all numbers have the property *p* is equivalent to saying : ' 1 has the property *p,* and, if *c* has it, then *c* + 1 has it '. Thus, such propositions depend on the fact that the whole nature of numbers is to form a sequence generated by arithmetical addition.

Mr. Kneale argues that all proofs of universal propositions about numbers involve mathematical induction. For propositions about

other kinds of number are reducible to complicated statements about integers, and all proofs of universal propositions about integers depend on mathematical induction. Proofs which seem *prima facie* to be independent of this process involve the principles of algebra, *e.g.*, the associative law, and these can be proved only by recurrence.

Ampliative Induction is concerned to establish natural laws and probability-rules. For the present we will confine our attention to the former. A law of nature is a proposition of the form : All S is P, where the description ' S ' applies to a potentially unlimited class of individual existents.

Mr. Kneale distinguishes the following four types of law. (i) *Uniform associations of attributes.* These are the laws which are involved in the existence of those groups of associated properties which mark out natural kinds. (ii) *Uniformities of development in natural processes.* Examples are found in the course of development of an embyro, of a chemical reaction, and so on. The Second Law of Thermodynamics is an advanced instance. (iii) *Laws of functional relationship.* An example would be the gas-law PV = RT. Such laws require that there shall be a uniform relationship between values of the several variables, and that this shall be express-ible in a formula of pure mathematics. (iv) *Numerical natural constants.* An example would be the law that gold melts at such and such a temperature. (It will be noted that each of the last three kinds of law involves a reference to natural kinds, *e.g.*, embryos of *mammals*, instances of *gases*, bits of *gold*.)

Mr. Kneale gives an interesting historical account of the senses in which the word ' cause ' was used by Aristotle, by Bacon, and by Hume and his continuator Mill. In this connexion he gives a critical account of the eliminative methods proposed by Bacon and by Mill for discovering ' the cause ' or ' the effect ' of a given pheno-menon. His general conclusion is that philosophers have tended to exaggerate the importance of the notion of cause in science. It is a vague notion, useful enough in some departments of practical life, but incapable of being made unambiguous and precise. When one tries, as Hume and Mill did, to tie it down to the notion of ' antecedent cause ', it develops ambiguities and difficulties ; and to describe science as a search for causes and causal laws, in this sense, is to give an inadequate and misleading account of the procedure of the more advanced sciences.

The most important section of this Part is concerned with the logical or ontological nature of laws. I shall expound Mr. Kneale's views and his reasons more fully later. For the present it will suffice to say that he considers and rejects the following views about natural laws, *viz.*, (i) that they are analogous to the restricted universals established by summary induction, (ii) that they are *facts* (as opposed to *principles*) of unrestricted generality, *i.e.*, the *de facto* regularity analysis, and (iii) that they are merely regu-lative prescriptions. Every alternative has its difficulties, but in the

7

end Mr. Kneale accepts and defends the view that laws are *principles of modality*, *i.e.*, are of the same nature as the propositions which are established by intuitive induction, although, for reasons which he gives, no law can be established in that way. This alternative, he says, is at any rate ' not entirely hopeless ', whilst all the others are so. It is ' the only account of laws which makes sense '.

There are hosts of alleged laws for which there is good inductive evidence, and serious science begins when we try to correlate and explain them. Such explanation may take two forms. (i) We may try to show that a large number of these laws follow logically from one or more others which have themselves been established by direct induction. (ii) We may try to show that they are entailed by one or more propositions which have not been, and from the nature of the case *could never be*, established by direct induction. Mr. Kneale calls the latter ' explanation by means of *Transcendent Hypothesis* '. An example of a transcendent hypothesis is the atomic theory or the wave-theory of light.

The peculiarity of a transcendent hypothesis is that the things and processes in terms of which it is formulated *could not* conceivably be perceived by the senses, and therefore, strictly speaking, could not be imagined either. It is plain that such hypotheses raise certain philosophical questions. Mr. Kneale's main answers are as follows :—

(i) Although we cannot imagine a transcendent entity, we conceive it as having a certain definite logical or mathematical *structure* embodied in a content which we cannot even conjecture. (ii) Any statement about a perceptual object, *e.g.*, a table, can be translated into statements about transcendent objects, *e.g.*, electrons and protons ; but there are many significant statements about transcendent objects, *e.g.*, about what happens inside an atom, which cannot be translated into statements about perceptual objects. (iii) Some of these non-translatable statements about transcendent objects are essential if the hypothesis is to explain known laws about perceptual objects and to suggest others which may be tested experimentally. (iv) For the above reasons Mr. Kneale rejects the view that statements about transcendent objects are merely a new and mathematically more handy terminology for talking about perceptual objects and their laws. He thinks that it would be unintelligible, on that view, that a transcendent hypothesis should enable one to infer laws about perceptual objects which had not as yet been established by direct induction. I do not find this argument very convincing. I suppose, *e.g.*, that the difference between the heliocentric and the geocentric descriptions of the planetary motions is merely a difference in the frame of reference adopted. Yet it is almost inconceivable that Kepler's laws of planetary motion would have been discovered unless the heliocentric description had been substituted for the geocentric ; and, unless they had been, it is almost inconceivable that the law of gravitation would have been discovered.

Mr. Kneale uses the term 'secondary induction' for the kind of reasoning by which a transcendent hypothesis, as distinct from an ordinary law about perceptual objects, is experimentally verified or refuted.

Suppose that a hypothesis H entails a number of laws, $L_1, L_2, \ldots$, for each of which there is direct inductive evidence, $e_1, e_2, \ldots$, respectively. Then each of these laws is supported indirectly by the direct evidence for all the others. For e_r, in supporting L_r, supports the hypothesis H, which entails L_r. And, in supporting H, it indirectly supports any other law, L_s, which is entailed by H. This is called by Mr. Kneale 'consilience of primary inductions'. It plays an important part in every advanced science.

The last topic dealt with in this Part is the relative importance of confirmation and elimination in induction. Mr. Kneale points out that elimination can lead to no positive conclusion unless it can be combined with *some* affirmative universal premiss. Now, even if some general principle of determinism could be formulated and were found to be self-evident, it would be far too abstract to serve as a useful premiss in an eliminative argument. In fact when scientists use such arguments they employ fairly concrete positive premisses, such as, *e.g.*, the proposition that all samples of a pure chemical substance have the same melting-point. Now these have to be established in the end by positive confirmatory inductive argument. So the fundamental problem of induction is confirmation by positive instances, and not elimination by negative instances.

(3) *The Theory of Chances.* Mr. Kneale defines a 'probability-rule' as a statement of the form : The probability of an instance of α being β is so-and-so. He symbolizes such a rule by the formula $P(\alpha, \beta) = p$. The calculus of chances is described as the procedure for deriving new probabilities from others which are given.

Mr. Kneale states the axioms and develops the theorems. All this is well done, but it raises no points of special interest. As might be expected, Mr. Kneale is under no illusions about the nature of Bernoulli's limit theorem, which he proves without using the differential calculus. He points out that it, like all theorems in the calculus of probability, merely derives one probability from another. On the other hand, it is a *necessary* proposition, and it is absurd to treat it as a law of nature which might be supported or refuted by experiments with coins or dice. It will be worth while, in this context, just to mention the notation which Mr. Kneale introduces for stating and proving theorems about the probability of a set of individuals having a certain composition. He uses the symbol $P(\alpha_{\sigma_n}, \beta_{\kappa_m})$ to denote the probability that a set of n instances of α contains exactly m instances of β. He uses a similar symbol, with β_{ρ_p} substituted for β_{κ_m}, to denote the probability that such a set contains a proportion p of instances of β.

I think that these symbols betray an inadequacy which was already latent in the notation $P(\alpha, \beta) = p$. What Mr. Kneale in fact wants

to symbolize is the probability that a set of n instances of α will contain exactly m instances of β, *given that* it is selected under certain conditions which might be called ' Bernoullian ', and *given that* the probability of an instance of α, so selected, being β is p. He has to state all this separately in words, and is unable to embody these conditions in his symbols. Yet, in the absence of some explicit reference to the first of these conditions, the symbol $P(^{\alpha}\sigma_n, \,^{\beta}\kappa_n)$ has no definite *meaning*; and, in the absence of some explicit reference to the second of them, it has no definite *algebraical form*, such as, *e.g.*, $^nC_m p^m (1-p)^{n-m}$.

Before leaving this part of my exposition I will mention that Mr. Kneale states and proves two interesting theorems of Poincaré's, one about the results of spinning a roulette-wheel, and the other about those of repeatedly shuffling a pair of cards. These he calls ' equalization theorems '.

The philosophically interesting contents of this Part begin in §32, where Mr. Kneale starts to investigate the Frequency Theory of the meaning of probability rules. He takes von Mises' form of this theory as the best available for discussion. This defines $P(\alpha, \beta)$ as the limit which the proportion of instances of β in a succession of instances of α approaches as the number of terms increases indefinitely, provided that the succession is of the kind which von Mises calls a ' collective '. This condition is that, if any endless sub-class be selected from the original succession, in accordance with any rule of place-selection, no matter how fantastic, the limiting proportion of β's in it will be the same as that in the original succession.

There are several well-known and obvious *prima facie* objections to this definition, and von Mises or his followers have attempted to answer them. Mr. Kneale gives a clear and fair account of these objections and the attempted answers. We may pass over this and consider what he has to say on his own account.

(i) The frequentists have often defended their notion of limiting frequencies by alleging that they are analogous to certain limiting notions which are constantly used in science, and to which no-one objects. Mr. Kneale complains that it is not clear what precisely they have in mind here. Is it the ideal figures of pure geometry in contrast with the imperfect straight lines, circles, etc., which occur naturally or can be constructed artificially ? Or is it such notions as frictionless fluids, perfect gases, and so on ? I should have suspected that it was neither of these, but rather the notions which are expressed by such phrases as ' density-at-a-point ', ' velocity-at-an-instant ', and so on. However this may be, Mr. Kneale objects that pure geometry is not a natural science and is quite indifferent to whether there are perfect circles, etc., in nature. Again, physicists know very well that there are no frictionless fluids or perfect gases. But the frequentists *define* such statements as ' $P(\alpha, \beta) = p$ ' in terms of collectives and limiting frequencies, and they believe that many probability statements apply within the actual world. Therefore they cannot

afford to be indifferent to the question whether there actually are collectives with limiting frequencies, still less can they afford to admit that there are none.

(ii) The definition of a 'collective' involves the notion of *laws* in the strict sense, *i.e.*, propositions of the form : Every instance of S (where the extension of S is potentially unlimited) is P.   But these laws are of a very odd kind, and it is very difficult to see why anyone should think he has good reason to accept them.   For they are of the form : *Every* infinite selection made by *any* rule of place-selection from the endless succession of α's contains the same limiting proportion of β's as the original succession.

(iii) The notion of a collective of α's in which the limiting proportion of β's is 1 covers two cases which common-sense sharply distinguishes.   One is that of law, *viz.*, Every instance of α is β. The other is the case where, although the limiting ratio is 1, yet there are many (it may be infinitely many) instances of α which are not β. If the frequentist thinks that he can get rid of the notion of law and reduce all instances of unitary probability to the second heading, it is plain from what has been said above that he is mistaken.

(iv) Consider, *e.g.*, the following application of Bernoulli's Theorem.   If the chance of throwing a 5 with a certain die is 1/6, then there is a very high probability that the percentage of 5's in a set of 1000 throws with that die is in the near neighbourhood of 16·66 per cent.   Now let us interpret this in terms of the frequency theory. It will run as follows.   If in an endless succession of *single throws* with this die the limiting ratio of the number of 5's to the number of throws is 1/6, then in an endless succession of *sets of 1000 throws* with it the limiting ratio of the number of such sets with about 16·66 per cent. of 5's in each of them to the number of such sets is not far short of 1.   Now would a knowledge of the antecedent proposition about the properties of an endless succession of *single throws* give you any good reason to bet on a non-5 rather than a 5 at *the next throw* ?   And would a knowledge of the consequent proposition about the properties of an endless succession of *sets of 1000 throws* give you any good reason to bet on a percentage of 5's near to 16·66 per cent. in *the next set of 1000 throws* ?   The answer in both cases seems plainly to be No.   Yet a satisfactory analysis of probability-rules ought to account for the fact that we think it reasonable to use them as guides to action in making decisions about particular cases and particular sets of many cases.

For such reasons as these Mr. Kneale rejects the frequency theory of the meaning of the probability-rules.

Mr. Kneale approaches his own theory of the meaning of probability rules by way of a discussion on the notions of Equiprobability and Indifference.   He rejects, on the usual and quite conclusive grounds, the Principle of Indifference, *i.e.*, that alternatives are equally probable relative to a person's state of information if he knows of no reason for accepting one rather than another of them.   But the fact

that this principle gives no satisfactory *criterion* for judging whether several alternatives are equiprobable does not show that it is a mistake to *define* the measure of a probability in terms of equiprobable alternatives.

In developing his own theory Mr. Kneale begins with the case of a characteristic which has a finite range of application, *e.g.*, the concept of undergraduate of Oxford in 1949. To say that two alternatives under such a concept are ' equipossible ' is equivalent to saying that either (i) both are *ultimate*, *i.e.*, non-disjunctive, relative to that concept, or (ii) that each consists of a disjunction of the same number of ultimate alternatives under it. An example under the first heading would be the alternatives of being this or that Oxford undergraduate in 1949. An example of alternatives which are not equipossible, relative to the sizes of the two colleges, are those of being an undergraduate of Christ Church or an undergraduate of Merton. If α is a characteristic with restricted application, the measure of $P(\alpha, \beta)$ is simply the ratio of the number of ultimate possibilities under [being-an-instance-of-$\alpha\beta$] to the number of ultimate possibilities under [being-an-instance-of-α]. *E.g.*, the chance that an Oxford undergraduate in 1949 will be an undergraduate of Christ Church is simply the ratio of the number of Christ Church undergraduates to the number of Oxford undergraduates in that year.

Mr. Kneale contrasts this with the indifference theory as follows. On his theory, in order that alternatives may be equipossible they must *be* indifferent in a certain way in relation to the characteristic under which they fall, whether this fact is known or believed or not. On the indifference theory the *person* who makes the judgment of equipossibility must be indifferent in a certain way in *his attitudes* towards them.

We can pass now to Mr. Kneale's account of the much more difficult and important case where α is a characteristic which applies to a potentially unlimited class of individuals, *e.g.*, the property of being a throw with a certain die. This seems to me to be much the most difficult part of the book, and I can only state in my own way what I believe to be Mr. Kneale's doctrine.

I shall begin by introducing the term ' specialization of a characteristic '. To be red is a specialization of being coloured ; it may be called a ' determinate ' specialization. To be a cat is a specialization of being a mammal ; it may be called ' specific ' specialization. To be red and round is a specialization of being red (and equally, of course, of being round) ; it may be called a ' conjunctive ' specialization. Any characteristic A can be conjunctively specialized by conjoining it with any other characteristic B which A neither entails not excludes. Similarly AB can be further conjunctively specialized by conjoining it with C, provided that it neither entails nor excludes C. (We must remember, in this connexion, that there is for Mr. Kneale no difference in principle between *nomic* entailment or exclusion, *e.g.*, water

cannot flow uphill, and entailment or exclusion of the phenomeno-logical or logical kind, *e.g.*, a surface cannot simultaneously be red and green all over.) Starting with any generic characteristic, we can think of it as first being specialized specifically till we come to the notions of the various lowest species under the genus. Then we can think of each conjunct in the notion of each lowest species being specialized by becoming perfectly determinate in every possible way. Finally, we can think of each perfectly determinate specialization of each such lowest specific specialization being conjunctively specialized by combining it conjunctively with every other characteristic which it neither entails nor excludes. In this way we conceive of a set of *ultimate* specializations of the original characteristic. This, if I am not mistaken, is what Mr. Kneale means by the *Range* of a character-istic. Any possible individual instance of a characteristic must be an instance of *one* and *only* one of the ultimate possibilities in its range ; and any two individual instances of it must be instances of *different* ultimate possibilities in its range.

Now at a certain stage in the descending hierarachy of increasingly specialized alternatives under a characteristic there will be alternatives which are *completely* specialized both specifically and determinately and can therefore be further specialized *only* conjunctively.

If I understand him aright, Mr. Kneale calls any such alternative a ' Primary ' alternative. Now suppose that α, α_2, . . . α_r, . . . are a set of mutually exclusive and collectively exhaustive *primary* alternative specializations of α. Since each is primary, any further specialization of any of them, *e.g.*, of α_r, must be of the form $\alpha_r\theta$, where θ is a characteristic which is neither entailed nor excluded by α_r, or, as we may say for shortness, α_r is ' contingent to ' θ. Suppose now that it were the case that every characteristic to which *any* of the alternatives α_1, α_2, . . . is contingent is a characteristic to which *all* of them are contingent. Then it is plain that to every specialization of any of these alternatives there would correspond one and only one specialization of each of the others. For any specialization of α_r must be of the form $\alpha_r\theta$ (since α is primary), where α_r is contigent to θ. But if α_r is contingent to θ, then any other alternative in the set, *e.g.*, α_s, will also be contingent to θ, by hypothesis. Therefore there would be a specialization $\alpha_s\theta$ of α_s, corresponding to the specialization $\alpha_r\theta$ of α_r. Plainly there could not be more than one. And, since all the alternatives in the set are primary, none of them can have any specializations which are not of this conjunctive form. It follows that any set of alternatives under α, answering to the above con-ditions, would subdivide the range of α into sub-ranges, each of which covers exactly the same number of ultimate specializations of α. Accordingly, Mr. Kneale gives the name ' *Primary* set of equipossible alternatives under α ' to any set of mutually exclusive and collectively exhaustive primary alternatives under α, which are such that *all* are contingent to any characteristic to which *any* is contingent. Given a set of *primary* equipossible alternatives,

it is of course easy to form sets of equipossible alternatives which are not primary, *viz.*, by taking as the new alternatives disjunctions of equal numbers of the old ones without overlapping, *e.g.*, $\alpha_1 \vee \alpha_2$, $\alpha_3 \vee \alpha_4$, ...

So far we have confined our attention to the range of a single characteristic α. But, if we wish to define $P(\alpha, \beta)$, we must now introduce a reference to β. The next stage is this. Suppose there is a primary set of equipossible alternative specializations of α, such that each of them either entails or excludes β. (In general some would entail it, and the rest would exclude it.) Now, if α_r entails β or if it excludes β, it is plain that the conjunction of α_r with any other characteristic θ will also entail or exclude β, as the case may be. Thus we might say that θ in the alternative $\alpha_r\theta$ is 'superfluous' in respect of its entailing or excluding β. If there is a set of equipossible alternative primary specializations of α, each of which either entails or excludes β, it is plain that there must be such a set composed of alternatives which are *minimal* in this respect, *i.e.*, which contain nothing superfluous to entailing or to excluding β, as the case may be. If I understand Mr. Kneale aright, he gives the name '*Principal* set of alternatives under α with respect to β' to a primary set of equipossible alternatives under α, each of which either entails or excludes β, and each of which is *minimal* in that respect.

At length we come to Mr. Kneale's account of the *meaning* of the statement '$P(\alpha, \beta) = p$'. If I am not mistaken, it is as follows. The *meaning* is the same in all applications, *viz.*, the ratio of the *measure* of the range of $\alpha\beta$ to the *measure* of the range of α. But in different types of application the ranges have to be measured in characteristically different ways. (i) If α determines a *closed* class, *e.g.*, contemporary Oxford undergraduates, then the measure of the range is simply the number of individuals in the class. (ii) If α determines an open *class*, *e.g.*, possible throws with a certain die, the first move is to introduce the notion of a primary set of equipossible alternative specializations of α, each of which either entails or excludes β. Two possibilities then arise. (*a*) Although the range of α is infinite, it may be that the principal set of equipossible primary alternatives under α with respect to β is finite. In that case $P(\alpha, \beta)$ is the ratio of the number of alternatives in this set which entail β to the total number of alternatives in it. (*b*) It may be that the principal set of equipossible primary alternatives under α with respect to β is itself infinite, *e.g.*, they may involve the different values of a *continuous* variable. Mr. Kneale says that, in such cases, the measure of a range has to be conceived as the measure of 'a region in a configuration-space', *i.e.*, by analogy with the length of a line or the area of a surface or the volume of a solid, and $P(\alpha, \beta)$ has to be regarded as the ratio between the measures of two such regions.

Mr. Kneale does not give us much help in connexion with the *general* notion of a configuration-space in probability or with the question how regions in it are supposed to be measured. He does

discuss very elaborately certain well-known paradoxes of 'geometrical' probability. His discussion of Bertrand's Paradox about the probability of a chord ' drawn at random in a circle ' being longer than the side of the inscribed equilateral triangle seems to me very illuminating.

Reverting to the general topic of the Range Theory, we must note that Mr. Kneale is perfectly well aware that no-one can produce an example of a principal set of equipossible primary alternatives falling under any natural characteristic, such as *human*. He is claiming only to *analyze the meaning* of ' $P(\alpha, \beta) = p$ '. He does not imagine that a knowledge of this will enable one to *determine the value* of $P(\alpha, \beta)$ *a priori* when α, e.g., stands for *human*, and β, e.g., stands for *male*. All probability-rules about open classes resemble laws of nature, in that they can be inferred only by ampliative induction. The Frequentists are quite right in saying that the evidence for such rules is observed frequencies. Their mistake is to hold that what is inferred is definable in terms of frequency. This mistake is analogous to that of thinking that a *law* is a 100 per cent. *de facto* association. The assumption at the back of both mistakes is that the conclusion of an inference must be a proposition of the same type as the premisses. If Mr. Kneale is right, the conclusions of all ampliative inductions are different in kind from their premisses. For the premisses are in all cases about *matters of fact :* whilst the conclusions, according to him, are *principles of modality*, whether they be laws or probability-rules.

The next important question discussed by Mr. Kneale is whether it can be shown, by means of the calculus of probability, that ampliative induction leads in favourable cases to conclusions which are highly probable in the sense contemplated by that calculus. After examining the so-called ' inversion ' of Bernoulli's Theorem, Laplace's Rules of Succession, and Keynes's Principle of Limited Variety, with unfavourable results, Mr. Kneale brings forward what he considers to be a fundamental objection to all attempts to justify ampliative induction within the theory of chances.

His argument may be put as follows. The propositions which we try to establish by ampliative induction are either laws or probability-rules. Let us begin with the laws. Suppose that the law to be established is All S is P. We have observed n instances of S, say $S_1, S_2, \ldots S_n$, and have found that all of them are P. It is claimed that we can show by using Bayes's Theorem that the probability that All S is P, given the conjunctive proposition S_1 is P-and-S_2 is P-and $\ldots S_n$ is P, approaches to 1 as n is indefinitely increased. Now it is admitted that the argument requires the fulfilment of the following two conditions. (i) That the *antecedent* probability of All S is P is greater than some number which is itself greater than 0. (ii) That the probability of the conjunctive proposition, given that the law is *false*, approaches indefinitely to 0 as n is indefinitely increased. It is argued that the second condition is fulfilled because

the probability of this conjunctive proposition, on this hypothesis, is the product of n terms, each of which is a proper fraction in a sequence whose successive terms do not tend to unity as n is indefinitely increased.   Now suppose, if possible, that the law All S is P were just an endless factual conjunction of singular propositions *i.e.*, that it was the proposition S_1 is P-and-S_2 is P-and . . . S_n is P-and . . . .   Then by precisely the same argument which proves that the second condition is fulfilled we could prove that the first condition is *not*.   On this interpretation of law the antecedent probability of any law would be 0.   Therefore, unless the argument is to break down at the first move, it must assume (what Mr. Kneale claims to have shown independently) that laws are *not* endless conjunctions of singular propositions.   This is the first step in Mr. Kneale's argument.

The next step is this.   The only acceptable alternative analysis of laws is that they are modal principles of necessary connexion between attributes.   But it is meaningless to assign a probability, in the sense in which that term is used in the theory of chances, to a modal principle.   Probability, in that sense, presupposes real objective alternative possibilities ; and it is plainly meaningless to regard a principle of necessary connexion as one alternative possibility among others.   Therefore a law has no antecedent probability (and of course no consequent probability) in the sense required by the above attempt to apply Bayes's Theorem.

Now, on Mr. Kneale's view, probability-rules are also modal principles concerning the possibilities that are left open by laws. Therefore they too can have no probability in the sense required in the theory of chances ; and therefore there can be no question of showing that the process of ampliative induction from observed frequencies confers upon probability-rules a high probability in that sense.

The last topic which Mr. Kneale discusses in this Part is the theory of sampling from finite populations.   Here the conclusion that the population as a whole contains a certain proportion of instances of a given characteristic has a probability in the sense required for the application of inverse-probability arguments.   But in practice such arguments are seldom applicable, since we do not generally know the antecedent probabilities of the various alternative possible proportions.

(4) *Probability of Inductive Science.*   The question discussed in this Part may be put as follows.   Is there any sense of ' justification ' in which ampliative induction *needs* justification ?   If so, *can* it be justified in that sense ?   And, if so, *how* can it be justified ?   The discussion is inevitably somewhat complicated.   For, in the first place, we have to deal with (1) *primary*, and (2) *secondary* inductions, *i.e.*, those which directly induce laws or probability-rules from observations and those which establish explanatory theories on the basis of such laws.   Then, within the discussion of primary induction, we have to consider the establishment of (1.1) *laws*, and (1.2) *probability-rules*.   Moreover, a law may be either (1.11) of the purely *qualitative*

form All S is P, or (1.12) of the *functional* form $Y = f(X)$. Lastly, the results of an inductive argument, whether primary or secondary, are not just rationally acceptable or unacceptable. According to circumstances they may be *more or less* rationally acceptable.

The ground has already been cleared to the following extent. We know that it is absurd to think that ampliative induction can be justified in the sense that its conclusions can be *deduced demonstratively* from its premisses. We also know that it is absurd to think that it can be justified in the sense that its conclusions can be shown to have a *high probability* (as understood by the theory of chances) in relation to its premisses. Some persons have concluded from this that the question : ' Is ampliative induction justifiable, and if so, how ? ' is a meaningless question which would cease to be asked if these negative facts were pointed out and appreciated. Mr. Kneale does not accept this conclusion. According to him, induction is a ' policy ' which one might or might not adopt in certain situations in which we are all very often placed. The question is whether we can show, apart from all reference to the truth or the probability (in the technical sense) of inductive conclusions, that inductive policy is the one which a sensible person, aware of his own needs, resources, and limitations, ' could not fail to choose '. I think that the phrase in inverted commas is highly ambiguous, and I am not perfectly sure what Mr. Kneale means by it. The meaning may become clearer to the reader when he has seen the application.

What then *is* the policy of primary induction (*a*) in regard to laws of the form All S is P, (*b*) in regard to laws of the form $Y = f(X)$, and (*c*) in regard to probability-rules ?

(*a*) Let us use the symbol ' S_0 ' to denote *observed* instances of S, and similarly *mutatis mutandis* for ' P_0 ' and ' Q_0 '. Suppose that the empirical facts can be stated as follows. All S_0 is P. All S_0 is Q. Some P_0 is neither S nor Q. Some Q_0 is neither S nor P. The only laws which are compatible with these observarions are All S is P, All SQ is P, All S is Q, and All SP is Q. The most timid policy would be to formulate no laws at all. Still playing for safety, one might formulate the laws All SQ is P and All SP is Q. The boldest policy consistent with the observations would be to accept tentatively the laws All S is P and All S is Q. The other aspect of the policy would be to look out for instances of S which were not P and instances of S which were not Q. But, unless and until such instances were found, it would be contrary to the policy to be content with the more restricted laws All SQ is P and All SP is Q. The policy here may be summed up as follows. (i) Act in all relevant circumstances on the assumption that combinations of characteristics of which you have found no instances in spite of seeking for them are incompatible. But (ii) continue to look for instances of such conjunctions, and be prepared to admit extensions of the range of what you have hitherto taken to be possible so far and only so far as fresh observations compel you to do so.

(b) The inductive policy in the case of functional laws is as follows. Act on the assumption that the law connecting the values of Y with the associated values of X is the 'simplest' consistent with the observations made up to date, but be on the look-out for new pairs of associated values which this curve fails to fit.   Here one curve is 'simpler' than another if it requires fewer independent parameters to determine it completely ; in this sense a straight line is simpler than a circle, a circle than a parabola, and a parabola than an ellipse or an hyperbola.

(c) In the case of probability-rules the inductive policy is as follows. If the relative frequency of instances of α which are β among all the instances of α which have been observed is p, act on the assumption that the value of P(α, β) is p.   What we are trying to do in such cases, on Mr. Kneale's interpretation of P(α, β), is to make the best guess that we can, on the basis of the available statistical evidence, as to the ratio of the range of possibilities under $\alpha\beta$, left open by all the principles of necessitation and exclusion, to the range of possibilities under α, left open by those principles.   It should be noted that to act on this policy is equivalent to assuming that value of P(α, β) which gives the maximum probability to the actual frequency of β's found in the finite class of n observed instances of α, *i.e.*, which maximises the value of P$(^\alpha\sigma_n, {}^\beta\rho_p)$.

The policy in all three cases falls under the following general maxim. In any case where you have to act, either practically or theoretically, on partial knowledge, act as if you knew that the boundaries of possibility lie as nearly as may be to the actual associations and dissociations and proportions which you have observed and critically tested up to date.

Why, and in what sense, is this policy ' reasonable ' or ' justifiable '? We are often in a position where our practical or theoretical interests oblige us to treat an object, of which we *know* only that it is or will be an instance of α, as if it were or would be β or as if it were or would be non-β.   The only way in which we can do this is by assuming the truth of a relevant law or probability-rule on the basis of our observations up to date. If all the observed instances of α have been β, it is for various reasons more profitable to assume the law that All α's are β than to assume any less sweeping law, such as All $\alpha\gamma$'s are β, or to assume merely that a certain percentage of α's are β.   The advantages are the following. If the supposition should be false, it is likely to be sooner refuted by counter-instances than any of the less sweeping suppositions compatible with the at present known facts.   If, on the other hand, it should be true, it will be more powerful as a premiss for inference than any of these less sweeping assumptions.   To this it may be added that, if one were to postulate anything but the strongest law consistent with the known facts, it is difficult to see where one could reasonably draw a line, since any set of observed instances of S which were all P would have innumerable properties in common beside S and P.

The justification is very similar in the case of functional laws. Suppose, *e.g.*, that you have observed n pairs of associated values of Y and X, and have found that they all fall on a certain straight line $y = a_0 + a_1 x$. The law connecting Y with X must be represented either by this straight line or by one of the innumerable curves of higher order which cut it in at least those n points but diverge from it elsewhere. If the linear hypothesis should be false, a single unfavourable further observation will suffice definitely to refute it ; but, however the $n + 1$th observation may turn out, it will be consistent with innumerable more complicated laws, between which one would have no reasonable ground for choosing.

I doubt whether I fully understand Mr. Kneale's argument to justify the procedure of assigning to $P(\alpha, \beta)$ the value p, when one has examined n instances of α and found that they contain a proportion p of β's. It certainly starts from the proposition (which is easily proved) that to assign any *other* value than p to $P(\alpha, \beta)$ would entail a *lower* value for the probability that a set of n instances of α would contain the observed proportion p of β's. The argument then seems to run as follows. By definition, the latter probability is the ratio of the range of possibilities under the property of being an n-fold set of α's containing a proportion p of β's to the range of alternatives under the property of being an n-fold set of α's containing *any* proportion of β's from 0 to 1. Now, it is alleged, the extent of the former range is *independent* of the value of $P(\alpha, \beta)$, whilst the extent of the latter range is *dependent* on the value of $P(\alpha, \beta)$. It follows that the value of $P(\alpha, \beta)$ which makes this ratio a maximum is the value which makes its denominator a *minimum*. Therefore, to assign as the value of $P(\alpha, \beta)$ the observed frequency p, with which instances of β have occurred in the n-fold set of α's examined, is equivalent to assuming that the range of possibilities under the property of being an n-fold set of α's containing *any* proportion of β's is as *narrow* as is consistent with the observations.

The step in this argument which I do not understand is the statement that the range of alternatives under the property of being an n-fold set of α's containing a proportion p of β's is *independent* of the value of $P(\alpha, \beta)$, whilst the range of alternatives under the property of being an n-fold set of α's containing *any* proportion of β's from 0 to 1 is *dependent* on the value of $P(\alpha, \beta)$. Let us take, *e.g.*, a finite class of N α's, and suppose it contains exactly Nq β's. Then the value of $P(\alpha, \beta)$ is q. Now the number of possible n-fold sub-classes containing a proportion p of β's would seem to be

$$Nq C_{np} \ ^{N(1-q)}C_{n(1-p)},$$

i.e., to be *dependent* on q, the value of $P(\alpha, \beta)$. And the number of possible n-fold sub-classes of *any* possible constitution in respect of β would seem to be $^N C_n$, *i.e.*, to be *independent* of q. This is the exact opposite of Mr. Kneale's statement. I suppose that there must be a simple misunderstanding somewhere, but I cannot make out where it lies.

The last topic to be discussed under this head is the varying degrees of irrationality which are involved in departing from the inductive policy under various circumstances. Here Mr. Kneale distinguishes two defects in a hypothesis, which he calls ' Extravagance ' and ' Negligence '. The former applies both to assumptions of law and assumptions of probability-rules. The latter applies only to the case of laws. I will take them in turn.

As we have seen, if we follow the inductive policy we are in effect ascribing to $P(\alpha, \beta)$ that value which maximizes the probability that an n-fold set of α's would have the proportion of β's which it has in fact been found to have. Mr. Kneale defines the ' extravagance ' of any departure from the inductive policy as the ratio of the diminution of this probability, entailed by that departure, to the maximal value, which it has if the policy is followed exactly. It is easy to show that, with this definition, the extravagance of a given departure from that value of $P(\alpha, \beta)$ which the inductive policy would dictate increases with the size of the sample observed. The formula covers the two extreme cases of 100 per cent. and 0 per cent. observed frequencies of β among α's, where the inductive policy would be to postulate a *law*.

' Negligence ', in the technical sense, consists in assuming only a probability-rule where the observations are consistent with a law ; or in assuming a law with a more restricted subject or a less determinate predicate when the observations are compatible with a law with a less restricted subject or a more determinate predicate.

So much for Mr. Kneale's views on the ' justification ' of primary induction ; it remains to consider the ' justification ' of secondary induction.

A theory is put forward to *explain* laws and probability-rules which have been or may be established by primary induction. A successful theory introduces *simplification* in two different, though connected, senses. In the first place, it must, of course, entail all the primary generalizations which it is put forward to explain, and others too which can be tested: Now it seems clear that the question whether a generalization, which is entailed by a theory, was established by primary induction *before* or *after* the putting forward of that theory cannot be of any logical relevance to the support which it gives to the theory. If a newly drawn consequence is to support the theory, it must be verified by primary induction before it can do so ; and, when once this has been done, it is in the same position as the already verified generalizations which the theory was originally put forward to explain. Mr. Kneale concludes that a theory is not worth serious consideration unless it entails an unlimited number of testable consequences. If this be granted, the first sense in which a successful theory simplifies is that it restricts the realm of possibility more than is done by any finite number of empirical generalizations entailed by it.

The second sense in which a successful theory simplifies is that it

reduces the number of independent concepts, and thus reduces the number of independent propositions, which we have to accept. An example is the unification of electricity, magnetism, light, etc., by Maxwell's Theory.

If the acceptability of a theory is to rest on its having been formulated and tested in accordance with a policy indispensable to pursuing an end which we seek, we must ask what that end is. Now theories certainly have the following two uses. A theory suggests subjects which it may be profitable to investigate by primary induction, and thus has an important directive use. Again, when it is shown that a number of primary generalizations are all consequences of a theory, the special evidence for each is reinforced by the evidence for all the rest. But, Mr. Kneale holds, these two valuable services which theories render are not the ultimate motive for theorizing by scientists. Men desire explanation for its own sake, and this desire is the main motive with pure scientists. The satisfaction derived from a good theory is in certain ways analogous to aesthetic satisfaction. But there are important differences. Scientific theorizing is not *free* construction, like musical composition. The scientist wants his theories to be *true*, and the minimum condition is that they shall be consistent with all known empirical facts. Moreover, he has the ideal of a single *all-embracing* theory, under which all possible empirical generalizations can be subsumed, and to which there is no alternative. Why men should have this ideal we do not know, but it is a fact that great scientists do have it. Secondary induction is justified in so far as it is the only policy by which we can set about realizing this ideal. We have no guarantee that it is realizable, and, if we happened to have realized it, we could never *know* that we had done so. But, if there *is* a single system of natural necessity, then the procedure of secondary induction is the only policy by which we can hope to approximate our beliefs to it.

(II) CERTAIN CHARACTERISTIC DOCTRINES OF MR. KNEALE. As we have seen, Mr. Kneale holds the following unfashionable views. (i) That laws of nature are principles of necessity, of the same nature as the proposition : A surface cannot be at the same time red and green all over ; though, unlike that proposition, they are incapable of being revealed by intuitive induction and known *a priori*. (ii) That such propositions are not merely linguistic. It will be convenient to consider his views on these two points in the opposite order to that in which I have stated them.

(1) *Principles of Modality are not merely linguistic.* Principles are truths about the possibility or impossibility of certain characteristics being combined in facts of a certain structure. They are more fundamental than facts, in the sense that it depends on them what are possible facts and what are not. On the other hand, we could not formulate any principle unless we were acquainted with, and had formulated, some facts. For, in the first place, we could not be aware of any characteristic unless we were acquainted with

facts in which it is a component. And, secondly, unless we had formulated some facts, we should have no means of symbolizing the structure of various kinds of possible fact. Mr. Kneale holds that all knowledge of singular negative facts, *e.g.*, the fact that the paper on which I am writing is not blue, involves knowing principles as well as facts. I must know, *e.g.*, the fact that this paper is white. But I must also know that it is possible for paper to be blue, and that being white all over is incompatible with being blue all over. This seems to me to be obviously true.

Consider now the allegation that the sentence ' It is impossible for anything to be at once red and green all over ' merely records a linguistic convention that no sentence of the form ' X is at once red and green all over ' is to be used. Certainly it is a matter of linguistic convention that ' red ' means what it does in English and that ' green ' means what it does in English. It is quite possible, *e.g.*, that ' red ' should have meant what it does now mean, and that ' green ' should have meant what is now meant by ' scarlet' or what is now meant by ' hot '. In that case the sentence ' X is at once red and green all over ' would have been permissible. The fact that it *is* not permissible depends on the fact that ' red ' and ' green ' at present mean two characteristics which are in themselves incompatible spatio-temporally. And it *would have been* permissible only if the meaning of one or of both of these words had been such that they name characteristics which are in themselves spatio-temporally compatible. *Any* language which contains names for the characteristics of which the words ' red ' and ' green ' are the names in contemporary English will have to use those words in accordance with a rule corresponding to the English rule about the use of ' red ' and ' green '. And that is because the rule states a principle concerning the characteristics of which these words are names. This, again, seems to me to be quite obviously true.

Mr. Kneale adds the following argument, which I give for what it may be worth. When one learns how to use a word, *e.g.*, ' red ', correctly, an essential part of what one learns is *not* to use it *unless* a certain condition C is fulfilled. In order to act on this knowledge one must be able to recognize cases in which C is *not* fulfilled. But one can never know a negative singular fact without using one's knowledge of a principle of incompatibility. Therefore ability to avoid using a word incorrectly involves knowing principles of modality.

(2) *Laws are Principles of Modality.* Mr. Kneale's view of the nature of laws may be compared with democracy in at least one respect. There are strong *prima facie* objections to it, and the only good arguments for it are the arguments against all the alternatives. Accordingly, we shall be concerned mainly with his criticisms of alternative analyses of law, and with his attempt to answer the *prima facie* objections to his own analysis of it.

The two alternative analyses which are worth serious consideration

are the following. (i) It might be alleged that the law : All S is P can be identified with the unrestricted factual proposition : Every instance of S that has been, is, or will be, has been P or is P or will be P, as the case may be. (ii) It has been alleged that laws, though expressed by sentences in the indicative, like ' All S is P ', are not really propositions at all. They are prescriptions, which would be less misleadingly expressed by a sentence in the imperative, *e.g.*, ' Whenever you meet with an instance of S and do not know whether it is P or not, act on the assumption that it is P '.

It has been objected to the purely factual analysis of law that, if it were true, no law could conceivably be verified by experience, and that this would entail that all nomic sentences are meaningless. Mr. Kneale does not accept this argument, because he rejects this criterion of significance. He points out that the statement ' There is at least one instance of S which is not P ' is certainly capable in principle of being verified, and is therefore significant by this criterion. It would be strange if this significant statement should have no significant contradictory.

Mr. Kneale's own objection is radical. Laws are not facts at all, and therefore not facts of the form alleged. To state a law properly we need a *conditional* sentence, not a mere sentence in the indicative. If it is a *law* that all S is P, then anything that *had* been, or that *might* now be, or that *should* in future be an instance of S *would* have been P, or *would* now be P, or *would* then be P.

Since nomic sentences are not statements of fact, anyone who denies that they are statements of modal principles of necessity, is practically forced to hold that they are not really statements at all but are disguised prescriptions. Now a prescription is either a command or an admonition. If Boyle's Law, *e.g.*, is a command, like ' form fours ', one would wish to know, before obeying it, who issues the command, what authority he has for doing so, and what penalties he can and will inflict in case of disobedience. Obviously there is no answer to these legitimate questions in the case of a law of nature. If on the other hand, it is an admonition, like ' Cast not a clout till May be out ', it is reasonable to ask what advantages are to be derived or what disadvantages are to be avoided by following the advice. If the person who gives us this advice answers that acting in this way will enable one to make successful predictions, he appears to be enunciating a law of nature in a non-prescriptive sense. If he answers that this is the policy which scientists do pursue, one can raise the following two supplementary questions. ' Do you mean merely to put on record the way in which scientists have in fact behaved up to date, or are you enunciating a *law*, in the non-prescriptive sense, about the behaviour of a certain class of human beings ? ' And whichever answer is given to this question, one can then ask : ' What is the relevance of your answer to the question why I should follow your advice in this matter ? ' To put it shortly, is there any reasonable ground for following the advice to act as if

8

S were P whenever you meet an instance of S except that there is reason to believe that : All S is P is a law of nature in the non-prescriptive sense ?

Finally, we can consider Mr. Kneale's answer to the *prima facie* objection that laws of nature cannot be principles of necessity, because any principle of necessity would be capable of being known *a priori* whilst no law of nature can be so known.

The objection is often put in the form that, if you can imagine an instance of S which is not P, then S cannot necessitate P. I shall state what I believe to be Mr. Kneale's main contentions in my own way and with my own examples.

In the first place, an example from pure mathematics has a certain relevance to the objection. Take the proposition that the square-root of 2 is irrational. This means that there are no two integers m and n, such that the ratio of m to n (reduced to its lowest terms) squared is equal to 2. Now this proposition is true and necessary and easily proved. But there is an important sense in which it is perfectly easy to ' imagine what it would be like ' if the proposition were false. One can imagine oneself applying to the number 2 the ordinary process for extracting a square-root, and finding that it came to an end after a finite number of steps, as it does, *e.g.*, after two steps if applied to the number 841. This example is useful as a counter-instance to the general principle that a proposition cannot be necessary if one can ' imagine what it would be like ' for it to be false. But it would be a mistake to rest any positive analogy on it ; for laws are certainly different in kind from propositions about numbers, even if they be of the same kind as propositions which can be established by intuitive induction.

Coming to Mr. Kneale's main contention, I find it easier to give an account of the explicit premisses, the main steps of the argument, and the conclusion, than to indicate the precise connexion between them. Mr. Kneale begins by pointing out that natural laws are concerned with *perceptual* events and things, *e.g.*, flashes of lightning or samples of ammonia, and not with merely *sensible* events and objects, such as the visual sense-datum which is presented to a person when he sees a flash of lightning or the olfactory sense-datum which is presented to him when he smells a whiff of ammonia.

He then considers the relation between sensation and sense perception. He accepts the conclusion that the statement ' X is seeing the perceptual-object O ' implies (i) that X is sensing a certain visual sense-datum, and (ii) that this, in some sense, ' belongs ' to a certain physical object which can be correctly described as, or named by, ' O '. In considering what meaning to attach to the word ' belongs ' in this context he rejects any view which would imply that it is intelligible to suggest that there might be a sense-datum which was not sensed by anyone. After considering and rejecting various alternative theories, Mr. Kneale says that he thinks that the following is ' correct so far as it goes '. Statements involving names and de-

scriptions of perceptual objects and their properties are not *reducible to* statements about actual and possible sensations ; but they are an *appropriate device* for referring briefly and compendiously to innumerable propositions about the sensations which would be experienced under innumerable different conditions. It must be noted, however, that an unlimited number of these propositions about sensations would be of the form : ' If a person *had* been in such and such a place at such and such a time and *had* then and there done such and such things, he *would* have had such and such sensations ', where no-one in fact was there or did those things at that time. Such propositions about the consequences of unfulfilled conditions seem to involve, either directly or at a later move, propositions to the effect that one kind of sensible experience would *necessitate* a sensible experience of a certain other kind.

Mr. Kneale concludes from all this (what is undoubtedly true) that perceptual-object words, like ' lightning ', ' ammonia ', ' flexible ', ' soluble in water ', and so on, obey utterly different rules from words and phrases about individual sense-data and their qualities. He suggests that the opinion that laws of nature would be knowable *a priori* if they were principles of necessity has arisen only because people have either failed to notice that such laws are concerned with perceptual objects and their properties, or have failed to see that propositions about the latter differ fundamentally from propositions about sense-data and their qualities.

Now it is this last vitally important contention which seems to me not to have been adequately developed and illustrated by Mr. Kneale. I think that he ought to have done the following three things. (i) To produce evidence that competent contemporary philosophers who disagree with his views on the nature of laws *do in fact* fail to see the distinction in question. For my part, I very much doubt that they do. (ii) To show us *why* principles of necessary connexion concerning sense-data and their qualities might be expected to be capable of being known *a priori*. And (iii) to indicate *how precisely* the admitted differences between sense-data and their qualities, on the one hand, and perceptual objects and their properties, on the other, make it impossible that any principle of necessary connexion concerning the latter should be known *a priori*. If Mr. Kneale has given an adequate answer to questions (ii) and (iii), I must confess that I do not understand it clearly enough to be able to convey it to the reader.

I greatly hope that Mr. Kneale will enlighten us further on these points. In the meanwhile he may be heartily congratulated and thanked for the bold, original, and extremely well-written contribution which he has made to one of the hardest and weightiest of the problems of philosophy.

C. D. BROAD.

4

VIII.—NEW BOOKS

The Philosophy of Francis Bacon. By FULTON H. ANDERSON. The
University of Chicago Press, Chicago, Illinois (Great Britain and
Ireland: Cambridge University Press), 1948. vii + 312. Pp. 22s. 6d.

THIS book is not an " examination " of Francis Bacon's philosophy, but
a "presentation" of it. The author lets Bacon speak for himself. His
chief task has been to select the statements and quotations from Bacon's
works and present them to the reader as an ordered whole. Considering
the heterogeneous and incomplete character of Bacon's writings, this
task has been far from easy. One willingly admits that the author has
mastered it admirably. He has presented the public with an easily
readable and clear account of Bacon's philosophy, which very few will have
patience and interest enough to extract for themselves directly from the
sources. When the presentation has made it necessary for the author
to enter on questions of interpretation or touch upon points of controversy,
he has succeeded to give solid and, in the main, convincing reasons for his
arguments and own views.

There are illuminating accounts of Bacon's philosophy of the scientific
method, *e.g.* by Ellis and Kotarbinski. Professor Anderson's account is
more detailed than that of most other commentators, but hardly presents
us with much new material of importance. The most valuable part of his
book are the chapters which deal with Bacon's materialism and his
" refutation " of previous philosophies. Particularly interesting is the
account which the author gives of the content of *De sapientia veterum*,
where Bacon in the form of interpretations of thirty-one ancient fables
advocates a revival of materialism and a complete separation of natural
philosophy from revealed theology. This book is not included in the
Spedding-Ellis edition of Bacon's *philosophical* works and, as the author
observes, "its almost complete neglect by commentators is among the
strangest phenomena in the history of philosophical exegesis ". Of
interest is also the short chapter reviewing Bacon's reflections on Plato,
whose ideas on method seem to have considerably impressed the great
philosopher of induction. Rather too summary is the chapter on Bacon
and the " post-aristotelians ", which term is taken to cover not only the
Stoics, the Epicureans, the Sceptics, and the Schoolmen but also
Copernicus, Galileo, Gilbert, Paracelsus, and Telesio.

As a " first systematic treatment of all Bacon's philosophic works "
Professor Anderson's book suffers from several limitations. Criticism,
however, is largely disarmed by the fact that most of these limitations are
intentional and self-imposed. The most serious of them seems to me to
consist in a certain lack of perspective caused by the almost total absence
of comparison of Bacon with his contemporaries in science and philosophy.
For the correct assessment of Bacon's place and significance in the history
of thought we need not only an account of what he said and thought, but
also a clear apprehension of what is missing in his philosophy. This
philosophy is a curious blend of abundance of brilliant ideas in certain
fields and almost complete barrenness in others where many of his great
contemporaries were fertile. He did probably more than any other single
individual has done to promote the logic of scientific method. But he

116

hardly contributed anything to the advancement of scientific ideas. He was one of the men who revived atomism and he was a champion of the idea of a mechanistic explanation of the phenomena of nature from their efficient causes. Yet the fundamental notions of mechanics which was destined to become the chief intellectual vehicle of this new type of science and philosophy, were completely obscure or unknown to him. And he failed to see clearly the importance of mathematics to applied science.

A bibliography of Bacon's extant philosophic writings and of the literature about his philosophy would have been useful.

G. H. von Wright.

Pythagoreans and Eleatics. An account of the interaction between the two opposed schools during the fifth and early fourth centuries B.C. By J. E. Raven. Cambridge University Press, 1948. Pp. viii + 196. 12s. 6d.

Mr. Raven's dissertation is concerned primarily with Pythagoreans; the Eleatic fragments are used rather for the light that he believes they throw on his main topic, the development of the notions of " Limit " and " Unlimited " and of the sense in which things were said to be numbers. Thus Parmenides appears as a reforming Pythagorean moved by " a basically religious desire to vindicate the good principle against the bad ", whereas the Pythagoreans themselves had from the earliest been dualists. Plato makes Zeno say (*Parm.* 128c) that his treatise is directed against those who poke fun at Parmenides' poem; this raillery was met first by the *tu quoque* from Zeno aimed against the plurality of units which the principle of Limit generated by " inhaling from the unlimited " ; secondly it was answered by Melissus' improvements on the Parmenidean One. The Pythagorean reaction to Zeno was to allow that a body is infinitely divisible, but still to claim that it " is " the number that is the number of points required to bound its surfaces. To this stage belongs the cosmogony of Ar., *De cælo*, II, xiii.

All this is highly speculative (not least the way in which " Socrates' " and " Zeno's " remarks in *Parmenides* are treated as history). And it is impossible here to discuss its relative merits as a wide view. It presents, however, a coherent picture, and there is little in it to shock.

But Mr. Raven is not an easy guide to follow. Indeed one seems like a mole to pursue an underground course through the rubble of Aristotelian interpretations, doxographers' misunderstandings and third hand quotations on which our conjectures about Pythagorean number theory have to be built. No doubt this is the best way to become acquainted with the composition of the road : but one is allowed too infrequent a glimpse of its direction and its place in the countryside. One reason outside the intrinsic difficulty of the subject is possibly this : the author's movements seem unduly hampered because he has started as though he were concerned only to refute Cornford's account of " number atomism " as the Pythagorean reply to Zeno. In fact his disagreement with Cornford is not so much over Pythagorean theory as over its order of development. Some sort of number atomism emerges again. Only it is put back *before* Parmenides ; and of course it is derived from the primary pair of contraries—Cornford's suggestion of a Pythagorean *monism* would have been refuted less elaborately by most scholars.

The value of the book is likely therefore to be found in suggestive details and in its quotation of sources, not all of which are in Diels-Kranz.

For instance an impressive parallel is revealed (pp. 158–63) between Theo Smyrnaeus' Tetractyes and the better known cosmogony preserved by Alexander Polyhistor. (For new proof, by the way, of the latter's antiquity see v. Fritz in *Class philol.* 1946, p. 33.) And the explanation (pp. 103–8) of what Eurytus was doing with his pebbles at least makes him less childish than is usually contrived. On the other hand the treatment of Parmenides (ch. iii) illustrates the pitfalls as well as advantages of the philological approach. "The Way of Truth" is admirably distinguished from "the Way of Seeming" in terms of *aistheta*, which require both columns of contraries, as distinct from *noeta*, which admit only one. But this will not do as an *explanation* (it is not quite clear that R. means it to be one), for it provides only an artistic, not a philosophical justification of the second half of the poem.

<div align="right">A. C. Lloyd.</div>

The Logic of the Sciences and the Humanities. By F. S. C. Northrop. New York : The Macmillan Company, 1948. $6 ; 22s. 6d.

This is a book whose preface is best read last, for its virtues, which are commendable though unspectacular, bear no relation whatsoever to the claims which the author and publisher make on its behalf.

To summarise its merits first : there is evidently one field with which Professor Northrop is comparatively familiar—the analysis of the problems and methods of the physical sciences. As long as he keeps to this field, he makes interesting and in some cases original suggestions. One must welcome, for example, his insistence that "there is no one scientific method " (ix), and that one must always analyse in detail the nature of any particular type of ' problem situation ' before prescribing the appropriate method of attacking it. He also reminds us (and this is worth noting) that "inquiry starts . . . when the facts necessary to resolve one's uncertainties are not known, when the likely relevant hypotheses are not even imagined " (17)—a fact of the utmost importance, which is too often forgotten in text-book discussions of ' scientific method '. He gives us a worthwhile discussion of Galileo's contribution to dynamics (22-29), though a series of uncorrected typist's errors (the text on p. 25 repeatedly reads ' force ' where ' fall ', or better ' velocity ', must be intended) make the exposition unintelligible at a vital stage. He also sees that concepts like ' electron ' cannot, as some have suggested, be explicitly defined in terms of everyday objects and operations, though one surely cannot be meant to interpret literally his suggestion that therefore "scales and clocks and Geiger counters have to be defined in terms of the more subtle and deductively fertile scientific objects " (113). He further discusses the differences between physics and the related sciences, arguing that economic theory cannot be expected to provide the sort of predictive power that dynamics gives to astronomy ; and writes interestingly on a variety of more strictly scientific subjects—such as quantum physics and embryology.

There is however one thing, in this part of the book, over which issue must be taken : Professor Northrop's uncritical tendency to confound philosophical issues which, both in origin and in nature, are entirely distinct. Thus he naïvely treats the ' sense data ' of the philosophy of perception as including as special cases both the ' empirical data ' of natural science and the ' direct visual experiences ' which artists of the Impression-

ist School professed to capture. Correspondingly, no distinction is made between the process of 'logical construction', in terms of which material objects have sometimes been tentatively analyzed, and those developments of scientific theory, which have added fresh terms of a more recondite nature ('electron' etc.) to our equipment. This tendency is even more noticeable in what follows.

Professor Northrop could have made a worthwhile book out of the more restricted essays and reviews which make up the greater part of the volume. It is a real pity that he should ever have been persuaded to do more—to identify *all* types of valid reasoning with 'scientific method', and to suppose that, by generalising the results of his study of physics, he would be in a position to prescribe for 'World Problems'. His publisher claims on his behalf that the book " opens a way to the scientific solution of social and ethical problems, and the possibility of reaching agreement in ideological controversy . . . outlines a complete scientific procedure for solving problems of value, and indicates how it may be applied to arrive at economic, political, æsthetic and religious theories that are valid for everyone ".

These are large claims. To show how far they are justified, let me quote some examples of the conclusions Professor Northrop reaches :

" What light does a philosophical analysis of the verified theories of natural science throw upon the nature of the correct normative social theory which must define our social ideal for the immediate future ? . . . Natural science exhibits the character of nature to us in two ways : one, through the immediately apprehended continuum . . . the other, through the systematically related unobserved entities and processes designated by the postulates of that verified theory. [These he calls the 'æsthetic' and 'theoretic' components of nature.] . . . We arrive, *therefore*, at this general concept of the idea of the good for our culture : That form of society is the good one which embodies in the emotions of men a sensitivity to nature in its æsthetic aspect and orders its education, its intellectual outlook, and its social institutions in the light of the latest verified, philosophically articulate scientific theory of nature in its theoretic aspect " (288-289). (My italics.)

" Conflicts between the delegates of the U.S.S.R. and the U.S.A. in the United Nations . . . cannot be resolved either by practical compromises alone or by a mutual understanding of each other's position and premises alone. . . . Only by learning to think deductively and thus becoming capable of formulating such an international conflict in precise theoretical terms, can the way out be found. . . . Similar problems occur continually in the deductively formulated experimentally verified theories of the natural sciences, and the technique for resolving them is well known. One must pass from the assumptions of both of the contradictory theories to a new set of philosophical premises which take care of the facts supporting the two contradictory theories, without contradiction " (320-321).

Professor Northrop might with profit have remembered his own advice, to fit the method to the problem, before prescribing for diplomats a method of attack so patently taken from physics.

On Education—" If natural science is to include and convey most effectively the qualitative, purely empirical part of its knowledge, it and our university teachers must use impressionistic art. For (this) is the instrument *par excellence* for conveying the purely

empirical data from which the generalizations of science and its attendant philosophy of culture are made " (326-327).

No one can say he does not take his Phenomenalism seriously !

His central conclusion—" That philosophy of culture, that normative social theory, is the scientifically verified and the correct one in which the basic philosophical primitive ideas and postulates are identical with the primitive ideas and postulates of the philosophy of natural science arrived at by the analysis of the verified theory of natural science which brings out into the open its basic methodological, epistemological and ontological assumptions " (342).

On Religion—Diagnosis : " The instruments for the control of technology for good ends are morality and religion. . . . A morality and religion which would control scientific technology must be one which can *connect itself* in some way with contemporary science. This, contemporary Western religion in both its Roman Catholic and Protestant forms, is incapable of doing. Roman Catholicism . . . is connected intimately with [out-of-date Aristotelian] science. . . . Contemporary Protestant doctrine on the other hand, can connect itself with no science whatever. This is because . . . modern Protestantism affirms that moral philosophy and religion are autonomous subjects. . . . For before one thing can hope to control another thing it must *connect itself* with that other. And this an autonomous morality and religion cannot do " (364-365). (My italics.) Prescription : " In addition to a knowledge of the Bible after modern biblical criticism has whittled down the statements of Christ to the few words which remain after the contributions of editors are removed, contemporary students [of theology] must be thoroughly trained in logic and in analysis of scientific method. For only thus . . . will the criteria be known for distinguishing trustworthy knowledge of unseen factors in man and nature [sc : God !] from untrustworthy theories of such factors. . . . A theistic religion, with such content and foundations, has some chance of directing the release of atomic energy to good ends, since being essentially connected with the theory and philosophy of contemporary science it has a way of effectively relating itself to the scientific technology which it would control " (382-383). As though religion controlled science as a rider controls his horse !

Two final plums : " St Paul was recognizing . . . that theism is not given by factual immediacy alone when he said, ' The things which are seen are temporal ; but the things which are unseen (the theoretically designated, scientifically known invariants, as opposed to the purely factually given, transitory sense data) are eternal ' " (46).

" From early childhood we have been conditioned in the face of a penalty of pain to instantly infer certain external objects from certain sense impressions. The child gets his hand slapped if he toys too long with the sensation of hotness and does not instantly infer the presence of a stove and respond in ways appropriate thereto " (112).

It may be true, as Professor Northrop complains, that neither logical analysis nor social science has done much so far to bring about ' agreement in ideological controversy '. This, however, is no accident, but reflects limitations on the scope of these subjects which must be recognised and accepted. Impetuous appeals (like Professor Northrop's) to

the highly provisional results of a limited branch of the philosophy of perception can solve no political problems. Instead they can bring only discredit upon philosophy and philosophers alike.

STEPHEN TOULMIN.

The Life of Reason : Hobbes, Locke, Bolingbroke. By D. G. JAMES. London : Longmans, Green & Co., 1949. Pp. xiii + 272. 18s.

THIS is the first of a series of four volumes on the English Augustans, *i.e.* the leaders of thought and letters over the period 1650–1780, with special reference to the poetry. In this volume Professor James has written only of Hobbes, Locke and Bolingbroke, and has attempted to show a connexion between their theories of knowledge and the current trends of literary criticism. Bolingbroke, though not a philosopher of the same rank as Hobbes and Locke, finds his place in the book because he had a direct influence on contemporary literature ; in particular Pope's *Essay on Man* is practically Bolingbroke put into verse.

Professor James' complaint against the Augustan philosophers is " that they failed to state the rightful place of the imagination in human experience ; that when they spoke of it, they wrongly opposed it, in its essential nature, to the intelligence ; and that, though it is abundantly true that the imagination and the intelligence create tensions in our experience, that does not mean that both alike may not take their places in the life of reason " (p. 270). Professor James wants to supplement the two Augustan faculties of sensation and understanding with a third, imagination. Here he has made an important point. Locke's language gives us no way of talking of such situations as seeing a puzzle picture *as* something or other, and there are important similarities between these and the poetic seeing of a ship *as* a portly merchant, for example, though Professor James has not sufficiently considered the differences. One cannot see a ship *as* a merchant until one can see a ship. If seeing a ship *is* imagination (*cf.*, p. 54) it is imagination in a different sense of " imagination " from that in which the poet's " seeing as " is imagination. Professor James has unfortunately not been alive to the dangers of talking in terms of " faculties ". Sensation, moreover, is presumably the faculty of having sensations, and these, we are told on p. 55, " are mere feelings saturated in emotion ". What sort of liquid is emotion ? Is an itch a mere feeling saturated in emotion ? And when I see something red have I a red in my eye in the way that I can have an itch in my toe ? Professor James is clearly a victim of the philosophical mythology about sensations which originated just before the Augustan era and is still thriving. Like Kant he does see that something is wrong with it, and much in the manner of Kant he patches the story up by putting in a further story about the imagination which supplements and orders these sensations and so gives the understanding something to think about. The story does not want patching, however, but scrapping.

When we pass to consider poetic imagination we pass to a different theme, which Professor James' faculty talk has prevented him from seeing to be different. This is the mapping of that part of language which is poetry, and Professor James is quite right in saying that Hobbes and Locke left no adequate place for this territory on their maps, though this is surely not surprising. Locke had a task of the first magnitude in showing the criteria appropriate to mathematics and physics and in exposing those

who professed to deduce matters of fact *a priori*, and it is too much to expect that he would have much time left for thinking out what sort of a thing poetry is. Professor James, however, has just this interest at heart, and he has some extremely interesting things to say about this topic, especially on pp. 142–152 and 158–167. He explains how poetry *legitimately* uses equivocation. He quotes from the beginning of *The Merchant of Venice* :—

> " Your mind is tossing on the ocean ;
> There, where your argosies with portly sail,
> Like signiors and rich burghers of the flood "

and says : " The intelligence may agree that an argosy is like a rich merchant and in so doing must also see differences between them ; but the labour of the imagination is, so far as it can, to apprehend an idea which somehow consists of the idea of both ship and trader " (p. 145). The effort of reading poetry is not inaptly described by Professor James as " a tension between the imagination and the intelligence ", and his discussion is most suggestive ; how much clearer it would become if he could restate it without using the words " imagination ", " intelligence ", and " idea ". (Professor James' use of " idea " is just as equivocal as Locke's, and he does not apply the rule, imperative for an expositor of Locke, of paraphrasing him without using this word. His treatment of Locke on abstract ideas is extremely hackneyed.) Professor James rightly stresses the analogies between our æsthetic activities and our intellectual ones ; we cannot indeed *say* what *King Lear* says in the way that we can say what Maxwell's electromagnetic theory says, but there is such a thing as " getting the hang of *King Lear* " and being able to come out with the appropriate comment or quotation much as there is the ability to make the appropriate application of the physical theory, and there are good and bad poems just as there are good and bad theories. This, I take it, is the sort of thing that Professor James means by saying that the understanding is an outgrowth from the imagination (p. 141) but it would have been more illuminating if he could have put this in a way less reminiscent of an anatomy book.

The book does not go so far as one would like in showing the relationship between philosophical theories and the contemporary literature (as opposed to the contemporary criticism of literature). To do this satisfactorily one must understand the peculiar nature of philosophical paradox. Locke's paradox " we can have no scientific knowledge of physical things " or (in its other form) " science can give no certainty " gets its sting from using " science " or " certainty " in an unusual way and from our not realising that this is so. In this way the direct effect of philosophy on literature is essentially due to misunderstanding, in a way that the effect of the Theory of Evolution, say, does not necessarily depend on a misunderstanding. I suspect that Professor James does not realise the nature of " science can give no certainty " and thinks of Locke as having found a weakness in *all* science much as one might find a weakness in a particular scientific theory. This, however, is a region in which the professional philosopher may be of help to the student of literature. Professor James has at any rate shown the converse : that the professional critic can be of considerable help to philosophers, quite apart from the special interest his book will have for students of Hobbes and Locke.

<div align="right">J. J. C. Smart.</div>

Conditioned Reflexes and Neuron Organisation. By J. KONORSKI. Cambridge University Press. Pp. xiv + 267. 18s.

PROFESSOR KONORSKI is a Polish physiologist who worked in Russia before and during the war. His book deals with the fact that ". . . . at the turn of the nineteenth century, two related branches of physiological science began to develop systematically and almost independently of each other. There was the physiology of the lower nervous activity . . . dealt with in the work of Sherrington . . . and the physiology of the higher nervous activity, in the Pavlovian teaching on conditioned reflexes ". Having been much in the countries of both schools and living between them Professor Konorski is in a good position to try to reconcile their two points of view. He makes a sustained attempt to show the features that are shared by the two schools. It is doubtful, however, whether a satisfactory scheme for understanding higher nervous activity can be based on the rather simple view of reflex action that he adopts. For example his only solution to the key problem of the nature of the conditioning process (the " plasticity " of the nervous system as he usefully calls it) is to postulate continual " formation and multiplication of synapses ", for which he gives no anatomical evidence. It is curious how in this field the physiologist calls in unknown details of morphology while the morphologist postulates undiscovered activities to explain a process that neither of them understands.

The most valuable feature of the book is that it gives an inside account of the conditioned reflex theory and its experimental foundations. This theory probably has an important place in the beliefs held in Russia to-day, and we have here a means of seeing what it involves. One cannot help noticing that the list of " mechanisms " postulated in the brain is nearly as long as that of the " phenomena " encountered. With postulated or hypothetical processes of excitation, inhibition, positive and negative induction and the " irradiation " of all of these it should be possible to describe almost any behaviour, even without the " top capability " and " top inhibition " to cover unusual situations. It is difficult to believe that such an epicyclic formulation has great practical value, but then our orthodox physiology is not much use for describing cerebral events either. Professor Konorski has done a valuable service by introducing eastern and western physiologists to each others ways of thought !

<div align="right">J. Z. YOUNG.</div>

Natural Philosophy of Cause and Chance. By MAX BORN. The Waynflete Lectures delivered in the College of St. Mary Magdalen, Oxford, in Hilary Term, 1948. Oxford : Clarendon Press, 1949. Pp. vii + 215. 17s. 6d.

THE theme of these lectures is the role of cause and chance in modern statistical physics. Although most of the technical details are presented in thirty-six appendices, forming nearly one half of the book, the general impression created by the lectures is that the distinguished author must have enjoyed a ' busman's holiday ' while the non-physicists in his audience were taken on an exhausting intellectual excursion over un-familiar territory. For those who can appreciate such a survey these lectures will prove both useful and delightful ; but, for those whose main interest is in the philosophical presuppositions and implications of modern

physics, they may seem at first sight to be disappointing. Nevertheless, they provide an opportunity of becoming acquainted with the personal point of view of one who has been at, or near, the centre of the main stream of research in theoretical physics during the past forty years, the first twenty, at least, of which already appear to us like a ' golden age '.

The final chapter, on *Metaphysical Conclusions*, and the appendices on *Multiple Causes, Economy of Thinking*, and *Concluding Remarks*, respectively, should be of particular interest to the philosophically minded. Commenting on Mach's principle of economy, Born makes the point that " A minimum principle like this has, as is well known to mathematicians, a meaning only if a constraining condition is added ". More interest, however, will probably be aroused by the author's lucid presentation of the fundamental difference in outlook between most contemporary theoretical physicists on the one hand and Einstein and Planck on the other, for the point at issue is philosophical. Extracts from two letters written by Einstein to the author in 1944 and 1947 are quoted. " You believe ", writes Einstein, " in the dice-playing god, and I in the perfect rule of law in a world of something objectively existing which I try to catch in a wildly speculative way ". As Born so wisely comments, these letters teach us the fact " that even an exact science like physics is based on fundamental beliefs ".

G. J. WHITROW.

De Methodo, ed opuscoli. By GIACOMO ACONCIO, ed. by G· Radetti. *Stratagematum Satanae*. By GIACOMO ACONCIO, ed. by G. Radetti. *La disputa delle Arti nel Quattrocento*. By G. BALDI, L. BRUNI, POGGIO BRACCIOLINI, etc., ed. by E Garin. Edizione nazionale dei classici del pensiero italiano, Nn. VI, VII, IX. Vallecchi, Firenze, 1944–1946–1947.

THIS collection of " classics of Italian thought " should be rather described as a collection of non-classical writers. Its real interest is not due to the *philosophical* importance of the authors represented, but rather to the presentation, in very accurate and even luxurious volumes, of less known texts which are important only as curious documents of the history of Italian thought during the Middle Ages and the Renaissance. Professor Nardi has edited here *opuscola* by St Pier Damiani ; Professor Garin, minor works by Pico della Mirandola and other Quattrocento writers ; Professor Radetti, the works of Aconcio. A next volume is announced, containing the *Theologia* by Campanella, and it will be the first one due to an Italian philosopher of note.

But Aconcio, although a minor thinker and a theologian, may have a special interest for English historians. Aconcio (or Contio or Acontio, lat. Acontius) was an Italian theologian who escaped from Italy in 1557 and in 1559 settled down in England, where he worked even as an engineer at the fortifications of Berwick, and died in 1567.

His major theological work, the *Stratagemata*, was dedicated to Queen Elizabeth and enjoyed a great popularity amongst Protestants. Of peculiar philosophical interest is his *De Methodo* in which he anticipates later developments in scientific theory and procedure.

Professor Radetti's accurate and valuable edition, with a complete bibliography, will be a real help towards a better appreciation of Aconcio's position in the history of Reformation.

MARIO M. ROSSI.

Filosofia e storiografia. By BENEDETTO CROCE. Laterza, Bari, 1949.
    Pp. 367. Lire 1400 (Saggi filosofici, XIII).

IN this latest book B. Croce collects essays on many arguments, mostly
of a highly controversial kind, which he has written in the last three years,
often under the immediate stimulus of contemporary trends and publica-
tions.

No completely new departure is to be expected from Croce after so
many years spent in defending the results he had attained thirty years ago,
by his " reformation " of Hegelian dialectics from which his thought
started half a century ago. During this long philosophical activity
Croce proved more than once that his basic conceptions could be main-
tained only by more or less evident twisting and turning, but *his* conception
of his own thought was never altered, and at this late stage he seems to
return with full confidence to the earliest stages of his philosophy. This
return is mostly due to the actual Marxist fashion in Italy and in other
countries, which stimulates him to repeat and renew his youthful criticism
of Marxism as the outcrop of an uncritical acceptation of Hegelianism
without any revision of its basic inconsistencies.

Croce repeats indeed that history is the supreme knowledge, and
philosophy only a historical methodology, and that historicism is the
great discovery of modern thought. In reviewing a book by one of his
adherents, he accepts to be considered as the representative of the fourth
and most perfect stage in the development of historicism.

On the other hand Croce proves again his positivistic respect for facts
and empirical data by fighting again and again against " philosophy of
history " or historical generalizations, and here he finds himself against
the problem of historical causation (see *e.g.* p. 176 *et passim*) which he
denies, although assessing the historical importance of certain events on
tne basis of tne *"ufficio"* (function) that such events exercised on the
course of history.

Croce remains always the hard-hitting polemist he always was, with all
his old, encrusted animosities. But a mellower tone now seems to prevail
in his conception of religion. Against his old habit of stigmatising as
" theological " what he disapproved of in philosophy and his repeated
declarations, that nothing in his philosophy should be construed as implying
a religious outlook, he now tries earnestly to account for Cnristianity
from a historicistic standpoint, and follows further some hints he gave a
few years ago when he tried to " superate " the contrast between free will
and Divine grace by an interpretation of both as different attitudes of
man towards history.

Whilst protesting against the current pessimism on the destinies of
mankind (he refuses to give up his historical optimism, and his idea of a
fatal progress of liberty in human history), he tries now to allow for
" radical evil " in human nature, by interpretating the Antichrist as a
permanent element in mankind. On the other hand he defends now the
conception of man as " redeemed by Christ's blood " against the nakedly
" economic " conception of men (pp. 234, 253), and defends even Catholic
religiosity against " Protestant " accusations of " paganism ".

                                                MARIO M. ROSSI.

Theory of Experimental Inference. By C. WEST CHURCHMAN. New York:
  The Macmillan Company; London: Macmillan and Co. Ltd.,
  1948. Pp. 292. 21s.

" THIS essay " writes Professor Churchman, " is really an attempt to
preach the gospel of modernism " : his sermon is addressed to the philos-
ophers and scientists, and is a plea for co-operation between them. To
be ' modern ' is to accept the value of recent developments in statistical
technique for all fields of enquiry ; the ' reactionaries ' are those who
believe that scientific enquiries can proceed from risk-free observational
records immune from statistical tests. The correct attitude for the
' experimentalist ' is to regard such records as ' postulates ' ; it must be
assumed that if the observations were continued indefinitely they would
approach a ' limit ' or ' ideal ' : the set of all such ideals is ' the real
world '. The assumption that these ideal limits ' exist ' is not arbitrary,
since " it is well motivated within our culture ". Professor Churchman
promises to elucidate this cryptic saying, but I cannot find that he has done
so, though he often insists that the assumption is a necessary condition of
scientific enquiry. It is a ' typically modern ' assumption, which alone
offers an escape from the infinite regress generated by the fact that the
answering of every question presupposes the answering of some other
question which presupposes. . . . It is, perhaps, a ' typically modern ' idea
that the logical obnoxiousness of an interminable regress can be annulled
by facing the other way and observing the prospect of an unending
progression.

  In some ways the central theme of this book is that no scientific problem
can be solved without the assistance of a ' science of ethics '. The argu-
ment, if I have not misunderstood it, is this : every scientific decision
involves the choice between a number of competing hypotheses ; what is
required, if this choice is to be freed from ' intuitions ', is that a definite
method be formulated for selecting ' the best ' hypothesis on the basis of
experimental tests. Now the statistician can advise us what the ' chances
of error ' are with any particular method of testing or sampling, but he
cannot tell us, qua statistician, what method to employ, since this depends
on the potential ' loss ' or ' cost ' entailed by the acceptance of a hypothesis
which may turn out to be false. The ' risk ' of accepting any hypothesis
is a function of the chance of error and the potential cost of a wrong
decision. If the potential cost is high, involving, say, the loss of life, no
decision will be acceptable for which the chance of error does not ' approach
zero ' ; if the cost is low, a decision may be acceptable even though the
chance of error be far from negligible. What is needed, then, to evaluate
alternative hypotheses is " a controlled science of losses and risks ; *i.e.* a
controlled science of ethics ". Risks and losses can only be assessed when
human needs and purposes are taken into account : thus, in any decision
affecting the welfare of a group the question would arise whether this
decision served the purposes of the group more efficiently than any other
decision. But in every society there exists a conflict of aims which can
only be resolved with the help of an ' experimental science of history ',
whose task it would be to reveal the ' predominant purposes ' common to
all societies in all ages. These, when found, would constitute the proper
(and eternal ?) criteria by which to measure scientific ' progress '.

  In a short review it is impossible to comment on this fantastic and
surely misconceived programme, which is left hopelessly vague at crucial
points. The guilelessness of this particular brand of pragmatism may be

illustrated by its dismissal of Hume's ' problem ' : we are told the problem only arises if the past is regarded as fixed and unalterable, whereas to a pragmatist all scientific results, whether of the past or future, are simply means to ends : " The ' past ' is no more ' predictable ' than the future, in the sense that what we take the past to be may or may not serve the ultimate end ". Evidently, if appeals to ' intuition ' are barred, the experimental science of history would require a prior enquiry to determine *its* ends, which in turn would require a predecessor . . . and so on, ends without end.

R. J. SPILSBURY.

The Ethics of Ambiguity. By SIMONE DE BEAUVOIR. Translated by Bernard Frechtman. Philosophical Library, New York, 1948. Pp. 163. $3.00.

THE difficulty for the existentialist moral philosopher, among whose mutually incompatible ancestors are Kant, Hegel, Kierkegaaid, and Husserl, is the derivation of the " ought " which he presses upon us from the " is " of his phenomenological description of the self, without the intermediary of a dogmatic metaphysic. (Such as Kant's ultimately *was*, the existentialist would say.) Simone de Beauvoir's little book, which might be described as a treatise on ethics, an exercise in pheno-menology, or a political pamphlet, does not confront this problem. She presupposes, rather than argues, Saitre's view of the self, and produces her imperatives therefrom without discussion. Freedom is the foundation of all value, man founds and maintains all the values he recognizes. The existentialist lesson is that, since he is the sort of being that he is, he ought to do this in " anguish ", with lucid consciousness of his responsibility, reaffirming his values, while knowing that they depend only on him. This position, as held by Miss de Beauvoir and other popular existentialist writers such as Camus, defines itself more as a polemical attack on the *esprit de sérieux* (the viewpoint of those who unquestioningly take their values as " given ", *e.g.* the bourgeois, the dogmatic party member), than as an attempt to work out a philosophical view of moral action. It is at this point that one most feels the slightness of Miss de Beauvoir's book. If freedom founds all values why *ought* I to will my own freedom and also (for this too is an imperative, in fact the fundamental one) the freedom of others ? Is " freedom " to be defined in terms of my attitude (anguish), or in terms of what I choose—and if the latter does this not involve a distinction between true and false values which cannot in turn be derived from the concept of free choice ? The existentialists may reply that they, like other moral philosophers, base their " how man should live " on their " what man is like ". (He is " naturally " an other-regarding being.) But some fuller account of moral reflexion and decision is needed to link these two, as well as a franker discussion of the " dogmatic " character of the Sartrian picture of the self. As it is we have Hegelian and Husserlian phenomenology on one hand and Kierkegaardian preaching on the other.

Miss de Beauvoir does not linger over these questions, but goes on to a diagnosis of political attitudes. (One sometimes has the feeling that she regards politics as the only kind of practical ethics.) She " psycho-analyses " various types of attitude to social relations (the sub-man, the nihilist, the adventurer etc.) and offers diagnoses of types of *mauvaise foi*, (the Communist who oscillates in argument between a dialectical and a

deterministic view of necessity).   These sketches are crude but it is worth
reflecting on what they attempt to do.   In a way they are " aids to moral
reflexion ", in a way they are quasi-proofs of the metaphysical position
adopted.   Is it a task of the moral philosopher to diagnose and describe
the attitudes and concepts in terms of which the moral conflicts of his age
are fought out ?   (Ethical concepts are *not* timeless.)   I would wish to say
yes.   But in what sort of language is this to be done, can it be done
" neutrally " ?   (Are Stevenson's descriptions of attitudes neutral ?)   Miss
de Beauvoir's descriptions are certainly not neutral, but are coloured both
by her metaphysical presuppositions and by certain passionate beliefs.

Perhaps the experience which has most coloured the author's approach
to ethics is that of the Resistance and of the post-war disillusionment ;
the " purity " of the moment of action followed by the hardening of a
living faith into " earnestness ".   The problems which seem to her most
important are those of mass political action, the relation of a man to his
party, and of the party to the people it serves ; the problem of how to win
freedom by violent means that temporarily deny it.   How is the Liberal
(or Christian) spirit of individualism to survive a long era of ideological
warfare ?   Miss de Beauvoir puts the problem with an admirable fierceness,
though her discussion of it is not prolonged or deep enough.   It is worth
noting that almost the only contemporary individual mentioned with
approval in her book is T. E. Lawrence.   Lawrence is an existentialist
hero because he was a man of action who kept his doubts alive.   (Compare
Rieux in Camus' " La Peste ".)   Should he be taken as the model of the
" good man " for this age ?   This question too is worth reflecting on.   The
Marxists are probably right in regarding the existentialists as the latest
theorists of Liberalism.   Lawrence was able to act in spite of his doubts ;
but most men are not Lawrences, and if they are to act they must put
doubt to sleep.   (Dialectical Materialism, as much as " bourgeois com-
placency ", excludes anguish.)   The Marxist will argue that in fact the
existentialist plays into the hands of the reactionary ; for the average man
constant reflexion hinders action.   The existentialist will reply that the
Marxist buys action at the cost of killing the power to reflect and losing
sight of the end.

The Marxist-Liberal debate, which is surely of immense importance for
any student of ethics or political theory, is sketched but not satisfactorily
discussed by Miss de Beauvoir.   She is inclined to take the notion of
" freedom " as not in itself problematic ; but can we really deduce our
political duties *now* from the command " set men free ", however much
in the nineteen-thirties we may have thought we could ?   The Marxist
and the Liberal views of the " free man " are not alike, and with this goes
a difference of value judgments into which Miss de Beaudoir does not enter.
She seems to assume (and is this so far from Kant's Kingdom of Ends ?)
that " ultimately " we shall all, if we act freely, be choosing compatible
things.

IRIS MURDOCH.

Four Views of Time in Ancient Philosophy. BY JOHN F. CALLAHAN.
Harvard University Press (London : Geoffrey Cumberlege), 1948.
Pp. ix + 209. 16s.

ONE merit of Professor Callahan's book is that it reminds us of Augustine's
work on the psychology of time and his analysis of the ' sense of duration '
and the concepts of past, present and future in terms of memory and

anticipation. But philosophically there is little to be said for the aim the author sets himself. This is to find in Plato, Aristotle, Plotinus and Augustine four different approaches to the ' problem of time ', corresponding to four particular ' methods of philosophical analysis ' which he labels ' metaphorical ', physical ', ' metaphysical ' and ' psychological '. His preoccupation with *the* ' problem of time ' won't commend him to those who prefer, like Aristotle, to discuss particular problems about temporal words rather than the question the author takes as his theme—' what time really is '. Nor do the four ' approaches to the problem ' really imply four different philosophical *methods* : the important difference is between two methods, analytical in Aristotle and (partially) Augustine and didactic in Plato and Plotinus. The statement that the four methods ' supplement one another in a most unusual way ' is otiose when we aren't told how their combined weight could be brought to bear on modern discussions of the problems involved. Professor Callahan doesn't inspire much confidence that he is familiar with these discussions. He mentions no modern philosopher (except, once, Kant : and indeed this neglect of the history of ideas seems characteristic of a treatment which discusses Plato's views on time without reference to Heraclitus). Another source of mistrust is the quantity of paraphrasing, which seldom avoids the ' dated ' flavour of a literal translation. On p. 91, for instance, we have " Thus eternity is not the substrate, but rather a kind of radiation of the substrate that goes forth from it in virtue of the identity it possesses in being that which is, not that which is to come ". The work may be of value as a series of commentaries and a comparative study, but not as an original contribution to the discussion of outstanding problems.

B. MAYO.

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9

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The Editor regrets to announce the death, at the age of 97, of Dr. J. N. Keynes. Dr. Keynes was not only one of the original members of the Mind Association; he was also a subscriber to MIND from its first issue. His ' Formal Logic ', which came out in 1884, will be known to many readers of MIND.

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HALL (Prof. E. W.), State University, Iowa City, U.S.A.
HALLETT (Prof. H. F.), 5 Lancaster House, Lansdowne Road, Worthing, Sussex.
HAMPSHIRE (S.), All Souls College, Oxford.
HAMPTON (Prof. H. V.), Aban Court Hotel, Harrington Gardens, South Kensington, London, S.W. 7.
HANSON-LOWE (J.), c/o Mrs. Hanson-Lowe, 20 Castelnau, Barnes, London, S.W. 13.

HARDIE (Prof. C. D.), The University, Hobart, Tasmania.
HARDIE (W. F. R.), Corpus Christi College, Oxford.
HARE (R. M.), Balliol College, Oxford.
HARRIS (Prof. Marjorie S.), Randolph-Macon College, Lynchburg, Va., U.S.A.
HARRISON (J.), Ryall Court, Albion Hill, Exmouth, Devon.
HART (H. L. A.), New College, Oxford.
HARTLAND-SWANN (Dr. J. J.), The University, Edmund St., Birmingham 3.
HARTSHORNE (Prof. C.), University of Chicago, Chicago 37, Ill., U.S.A.
HARTT (Prof. J.), 409 Prospect Street, New Haven, Conn., U.S.A.
HARVEY (Prof. J. W.), The University, Leeds.
HASEROT (Prof. F. S.), R.F.D. 1-39, Williamsburg, Va., U.S.A.
HAWKINS (Rev. D. J. B.), 19 Millbourne Lane, Esher, Surrey.
HAWKINS (G. J.), 7 Warwick Rd., Newport, Mon.
HEADLY (L. C.), House on the Hill, Woodhouse Eaves, Loughborough.
HELSEL (Prof. P. R.), 5502 Rimpan Blvd., Los Angeles 43, Cal., U.S.A.
HENDERSON (G. P.), 13 South Bridge Street, St. Andrews, Fife, Scotland.
HENDERSON (Prof. T. G.), 1556 Summerhill Avenue, Montreal, Canada.
HENLE (Prof. P.), Department of Philosophy, Northwestern University, Evanston, Ill., U.S.A.
HENRY (Prof. C. F. H.), Fuller Theological Seminary, P.O. Box 989, Pasadena, Cal., U.S.A.
HENSHAW (Rev. T.), 7 Adria Road, Manchester 20.
HERBST (P.), Department of Philosophy, The University, Melbourne, Australia.
HERD (W. L.), 10 Russell Avenue, Bedford.
HICK (J. H.), 5a Fulford Rd., Scarborough, Yorks.
HIRST (R. J.), Department of Philosophy, University College, Dundee, Scotland.
HOCKING (Prof. W. E.), Madison, N.H., U.S.A.
HODGSON (Prof. J. B.), 516 23rd Street, S.E., Cedar Rapids, Iowa, U.S.A.
HOFSTADTER (Prof. A.), 127 West 96th St., New York 25, U.S.A.
HOLMES (E. R.), 84 Commercial Road, Bulwell, Nottingham.
HOLT (D.), Engo, Riversdale Road, Liverpool 19.
HOOK (Prof. S.), Department of Philosophy, New York University, Washington Square College, Washington Square, N.Y., U.S.A.
HOSPERS (Dr. J. J.), 320 Folwell Hall University of Minnesota, Minneapolis 14, Minn., U.S.A.
HUGHES (G. E.), Dept. of Phil., University College, Bangor, N. Wales.
HURWITZ (H. M. B.), 3 Ashridge Way, Morden, Surrey.
HUTCHISON (A. J.), 5 Cornwall Avenue, London, N. 22.

JACK (P. J. H.), 70 Findhorn Place, Edinburgh 9.
JEFFREY (P. J.), 134 Ocean St., Narrabeen, near Sydney, N.S.W., Australia.
JESSOP (Prof. T. E.), University College, Hull.
JOHNSON (A. H.), Dept. of Phil., Univ. of Western Ontario, London, Canada.
JOHNSON (Rev. G.), 706 Nottingham Road, Wilmington 56, Del., U.S.A.
JOHNSTONE (R. E.), Apple Trees, Church Road, Purley, Surrey.
JOLLY (H. J.), Whitemay, Louvain Way, Garston, Watford, Herts.
JONES (A. L.), 26 Gledhow Valley Rd., Leeds 8, Yorks.
JONES (Dr. J. R.), Tanygraig, Trevor Road, Aberystwyth, Wales.
JØRGENSEN (Prof. J.), Parkvænget 15, Charlottenlund, Denmark.

KALISH (D.), 10446 Hebron Lane, Los Angeles 24, Cal., U.S.A.
KAPLAN (Prof. A.), Dept. of Phil., U.C.L.A., Los Angeles, California, U.S.A.
KAUFMANN (Prof. F.), 533 West 232nd Street, New York 63, N.Y., U.S.A.
KEBLE (E. R.), Grayrigg, Bishops Down Park Rd., Tunbridge Wells, Kent.
KEENE (G. B.), 57 Selly Park Rd., Birmingham 29.
KEMP (J.), Department of Moral Philosophy, The University, St. Andrews, Fife.
KNEALE (Mrs. M.), Lady Margaret Hall, Oxford.
KNEALE (W. C.), Exeter College, Oxford.
KNEEBONE (G.), 6 Second Avenue, Dovercourt, Harwich, Essex.

KNOX (Prof. T. M.), The University, St. Andrews, Fife, Scotland.
KÖRNER (Dr. S.), Department of Philosophy, The University, Bristol 2.
KURILOFF (Miss A. E.), 1050 East 15th St., Brooklyn 30, New York, U.S.A.
KYDD (G. R. M.), Cruglais, Swyddffynnon, Ystrad Meurig, Cards. *Life Member.*

LAMONT (W. D.), 83 Oakfield Avenue, Glasgow, W.2.
LANG (S.), 49 Wiggins St., Princeton, N.J., U.S.A.
LEAN (M. E.), Department of Philosophy, University of North Carolina, Chapel Hill, N.C., U.S.A.
LEAVENWORTH (Mrs. I.), 277 Park Avenue, New York 17, N.Y., U.S.A.
LECLERC (Dr. I.), 59 Gloucester Place, London, W. 1.
LEROY (A.), 6 Rue Albert Sorel, Paris 14eme, France.
LEVETT (Miss M. J.), The University, Glasgow. *Life Member.*
LEWIS (Prof. C. I.), Emerson Hall, Harvard University, Cambridge, Mass., U.S.A.
LEWIS (Prof. H. D.), University College of North Wales, Bangor.
LEWIS (Rev. T. A.), St. David's College, Lampeter, Cardiganshire.
LEWY (Dr. C.), The University, Liverpool 3.
LEYDEN (W. von), Hatfield College, Durham.
LIBRARIAN (The), Bedford College, Regent's Park, London, N.W. 1.
LIBRARIAN (The), Gibson Library, The University, Melbourne, Australia.
LIBRARIAN (The), Girton College, Cambridge.
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LIBRARIAN (The), Manchester College, Oxford.
LIBRARIAN (The), Pusey House, Oxford.
LIBRARIAN (The), The University, Jerusalem, Palestine.
LIBRARIAN (The), Lincoln College, Oxford.
LIBRARIAN (The), St. Xavier's College, Cruikshank Road, Bombay.
LIBRARIAN (The), Society of Sacred Mission, Kelham, Newark, Notts.
LIBRARIAN (The), University College, Cork, Ireland.
LIBRARIAN (The), University of Toledo, 2801 West Bancroft Street, Toledo 6, Ohio, U.S.A.
LIBRARIAN (The), Wake Forest College, Wake Forest, N.C., U.S.A.
LIEB (I. C.), 218 Goldwin Smith Hall, Cornell University, Ithaca, N.Y., U.S.A.
LILIENFELD (R. H.), 268 King's Highway, Brooklyn 23, N.Y., U.S.A.
LILLIE (Dr. R. A.), 267 Braid Road, Edinburgh.
LINDSAY (Rt. Hon. Lord), Keele Hall, Stoke-on-Trent, Staffs.
LLOYD (A. C.), The University, St. Andrews, Scotland.
LONG (Prof. W. H.), 10328 Bannockburn Drive, Los Angeles 34, Cal., U.S.A.
LOWE (Prof. V.), Box 122, Johns Hopkins University, Baltimore 18, Md., U.S.A.
LUCAS (P. G.), Department of Philosophy, The University, Manchester 13.
LUCE (Rev. Canon A. A.), Ryslaw, Bushy Park Road, Co. Dublin, Ireland.
LUTOSLAWSKI (Prof. W.), Szwedzka 10, Krakow, Poland.

MABBOTT (J. D.), St. John's College, Oxford. *Life Member.*
MACBEATH (Prof. A.), Queen's University, Belfast.
MACCARTHY (F. L.), 9 St. Cross Road, Oxford.
McCLUNG (M.), 2-584 Abbercorn Avenue, Town of Mount Royal, Quebec, Canada.
McCRACKEN (Dr. D. J.), The University, Edmund Street, Birmingham.
MacDONALD (Miss M.), 92 Holbein House, Sloane Square, London, S.W. 1.
McINTOSH (G. F.), Department of Philosophy, New England University College, Armidale, N.S.W., Australia.
MacIVER (A. M.), University College, Southampton.
MACKAY (Prof. D. S.), The University of California, Berkeley, Cal., U.S.A.
MACKENZIE (Dr. M.), 86 Harley Street, London, W. 1.
McKENZIE (Prof. L. W., Jnr.), Department of Economics, Duke University, Durham, N.C., U.S.A.

McKEON (Prof. R. P.), The University of Chicago, Chicago 37, U.S.A.
McKIE (J. I.), Brasenose College, Oxford.
MACLAGAN (Prof. W. G.), 6 The University, Glasgow, W. 2.
MACLENNAN (Prof. R. D.), Department of Philosophy, McGill University, Montreal, Que., Canada.
MacMURRAY (Prof. J.), The University, Edinburgh.
MACNABB (D. G. C.), Pembroke College, Oxford.
McPHERSON (T.), University College, Oxford.
MALCOLM (Dr. N.), Dept. of Phil., Cornell University, Ithaca, N.Y., U.S.A.
MALLET (E. H.), 14 St. James's Square, Bath.
MARDIROS (A. M.), Department of Philosophy, University of Alberta, Edmonton, Alberta, Canada.
MARHENKE (Prof. P.), Box 52, Wheeler Hall, University of California, Berkeley, Cal., U.S.A.
MARSH (D. R.), 25 Albury Drive, Pinner, Middlesex.
MARSH (Rev. J.), Mansfield College, Oxford.
MARSH (Prof. R.), 12 Mellen St., Cambridge, Mass., U.S.A.
MARSHALL (Prof. J. S.), The University of the South, Sewanee, Tenn., U.S.A.
MARTIN (Prof. R. M.), Department of Philosophy, University of Pennsylvania, Philadelphia 4, Pa., U.S.A.
MARVIN (Prof. E. L.), Montana State University, Missoula, Mon., U.S.A.
MASANI (B. M.), Evergreen, Gholvad, Thana District, Bombay Province, India.
MASLOW (Dr. A.), Dept. of Phil., Univ. of British Columbia, Vancouver, Canada.
MATTHEWS (D. G. J.), University Union, College Road, Newcastle-on-Tyne 2.
MAYO (B.), The University, Edmund Street, Birmingham 3.
MAYS (W.), Department of Philosophy, University of Manchester, Manchester 13.
MEAD (F. C.), 27 The Drive, Sevenoaks, Kent.
MAYERSON (Miss H. C.), 29 Washington Square W., New York 11, U.S.A.
MILES (T. R.), Riverside, Middleton St. George, Darlington, Co. Durham. *Life Member.*
MILLER (Dr. E.), 7 Devonshire Place, London, W.1.
MILLS (J. F.), 609 Hawthorne Avenue, Royal Oak, Michigan, U.S.A.
MINAS (J. S.), 834 Calumet Ave., Apt. 11, Detroit 1, Michigan, U.S.A.
MINIO-PALUELLO (Dr. L.), 20 Hernes Road, Oxford.
MITCHELL (A. H. M.), Carrick, 10 Wolsey Road, Moor Park, Northwood, Middlesex.
MITCHELL (G. D.), Barton Farm House, Dartington Hall, near Totnes, S. Devon.
MOFFATT (J. L.), Kirwee, Canterbury, New Zealand.
MONINS (I. R.), Ringwould House, Ringwould, near Dover, Kent.
MOORE (Prof. A. M.), Department of Philosophy, North Western University, Evanston, Ill., U.S.A.
MOORE (Prof. E. C.), Department of Philosophy, Iowa State University, Iowa City, Ia., U.S.A.
MOORE (Prof. G. E.), 86 Chesterton Road, Cambridge. *Honorary Member.*
MOORE (Prof. J. S.), Western Reserve University, Cleveland, Ohio, U.S.A.
MORGAN (Dr. B. S.), Oss-Omgus, A.P.O. 742, c/o Postmaster, New York, U.S.A.
MORRIS-JONES (H.), University College, Bangor, Caernarvonshire.
MORROW (Prof. G. R.), 310 Bennett Hall, University of Pennsylvania, Philadelphia, U.S.A.
MOTT (C. F.), 54 Strand-on-the-Green, Chiswick, London, W. 4.
MUNDLE (C. W. K.), Department of Philosophy, University College, Dundee.
MUNITZ (Prof. M. K.), Department of Philosophy, New York University, New York 3, N.Y., U.S.A.
MURDOCH (Miss I.), 4 Eastbourne Road, Chiswick, London, W. 4.
MURE (G. R. G.), Merton College, Oxford.
MURRAY (A. R. M.), 15A London Lane, Bromley, Kent.
MURRAY (Principal J.), University College, Exeter.

NAGEL (Prof. E.), Department of Philosophy, Columbia University, New York, N.Y., U.S.A.

NASON (Prof. J. W.), Swarthmore College, Swarthmore, Pa., U.S.A.
NELSON (Prof. E. J.), University of Washington, Seattle, U.S.A.
NICOLSON (H. D.), 66 Irvine Street, Kingsford, N.S.W., Australia.
NIKAM (N. A.), 28 Third Cross Road, Basavangudi, Bangalore, India.
NOALL (A. W.), 8 Winton Road, Victoria Docks, London, E. 16.
NORTHROP (Prof. F. C. S.), 1891 Yale Station, New Haven, Conn., U.S.A.
NOWELL SMITH (P. H.), Trinity College, Oxford.

OAKLEY (Miss H. D.), 22 Tufton Court, Westminster, London, S.W. 1.
O'CONNOR (D. J.), Department of Philosophy, University of Natal, Pieter-
maritzburg, South Africa.
ODERBERG (I. M.), 33 Beach Avenue, Elwood, S. 3, Melbourne, Australia.
OLIVER (Prof. J. W.), 511 E. Boulevard, Gainsville, Florida, U.S.A.
ORR (S. S.), Dept. of Phil., University of Melbourne, Victoria, Australia.
OSBORN (Sir F.), Mountcoombe Hotel, Oak Hill Grove, Surbiton, Surrey.
PANUSH (I.), 9603 No. Martindale Avenue, Detroit, 4, Michigan, U.S.A.
PAP (Prof. A.), Department of Philosophy, College of City of New York,
New York, N.Y., U.S.A.
PARSONS (Rev. R.), The Rectory, South Hackney, London, E. 9.
PATON (Prof. H. J.), Corpus Christi College, Oxford.
PATON (M.), 27 Beaconsfield Road, St. Albans, Herts.
PAUL (G. A.), University College, Oxford.
PEACH (W. B.), 33 Healey Street, Cambridge 38, Mass., U.S.A.
PEARS (D. F.), 1 Brewer Street, Oxford.
PERCY (Rev. J. D.), Little Orchard, West Hill, Ottery St. Mary ,Devon.
PERRY (J. W.), 19 Newark Way, Hendon, London, N.W. 4. *Life Member.*
PETERS (R. S.), Upper Orchard, Thaxted, Essex.
PHILLIPS (I. W.), 6 Lothian Gardens, Glasgow, N.W.
PICKARD-CAMBRIDGE (W. A.), Worcester College, Oxford.
POPPER (Prof. K. R.), London School of Economics, Houghton Street, London,
W.C. 2.
PORTEOUS (Prof. A. J. D.), 3 Kingsmead Road North, Birkenhead, Cheshire.
PORTER (N.), Flat 2, 53 Foam Street, Elwood, S. 3, Melbourne, Australia.
PRICE (Prof. H. H.), New College, Oxford.
PRIOR (A. N.), Philosophy Department, Canterbury University College,
Christchurch, New Zealand.

RAINER (A. C. A.), Dept. of Phil., King's College, Newcastle-on-Tyne.
RAMSAY (Rev. I. T.), Christ's College, Cambridge.
RANADE (Prof. R. D.), 15A Hastings Road, Allahabad, India.
RANKIN (K. W.), 7 Mayfield Terrace, Edinburgh 9.
RAPHAEL (D. D.), Department of Moral Philosophy, The University, Glasgow.
REES (D. A.), Department of Philosophy, University College of North Wales,
Bangor, Caernarvonshire.
RÉGNIER (Rev. M.), Archives de Philosophie, Val près Le Puy, Haute Loire,
France.
REID (Prof. L. A.), Institute of Education, University of London, Malet
Street, London, W.C. 1.
REINHARDT (J. F.), 5718 Harrison Street, Kansas City, 4, Mo., U.S.A.
RHEES (R.), 96 Bryn Road, Swansea, Glam.
RICE (V. I.), Commonwealth Bank of Australia (Strand Branch), Australia
House, Strand, London, W.C. 2.
RICHARDSON (C. A.), 9 Orchard Lane, West Wimbledon, London, S.W. 20.
RICHES (P.), Via Pietro Maestri 2, Milano, Italy.
RITCHIE (Prof. A. D.), Old Cottage, The University, Edinburgh.
ROBERTSON (J. H.), Masonic Villa, Ratho, Midlothian, Scotland.
ROBINSON (D. H.), 4 Linden Avenue, Maidenhead, Berks.
ROBINSON (Prof. D. S.), School of Philosophy, University of Southern Cali-
fornia, Los Angeles 7, Cal., U.S.A.
ROBINSON (R.), Oriel College, Oxford.
ROLFE (G. E.), 1311 North 79th Street, Seattle 3, Washington, U.S.A.

ROLLINS (C. D.), St. John's College, Oxford.
ROSE (Dr. Grace L.), Wheaton College, Norton, Mass., U.S.A.
ROSS (Prof. G. R. T.), 1 Townsend Drive, St. Albans, Herts.
ROSS (Prof. R. G.), Div. Gen. Ed., New York University, Washington Square, New York 3, N.Y., U.S.A.
ROSS (Sir W. D.), 17 Bradmore Road, Oxford.
ROTH (Prof. H. L.), The University, Jerusalem, Palestine.
RUSSELL (Rt. Hon. Earl), Trinity College, Cambridge.
RUSSELL (Prof. L. J.), 304 Hagley Road, Edgbaston, Birmingham 17.
RYLE (Prof. G.), Magdalen College, Oxford.

SALMAN (H.), Le Saulchoir, Soisy-sur-Seine, Seine et Oise, France. *Life Member*.
SAW (Miss R. L.), 72 Grosvenor Avenue, Carshalton, Surrey.
SCHARFSTEIN (Ben-Ami), 310 W.105th Street, New York 25, N.Y., U.S.A.
SCHNEIDER (Prof. H. W.), Philosophy Hall, Columbia University, New York, U.S.A.
SCHRECKER (Prof. P.), Swarthmore College, Swarthmore, Pa., U.S.A.
SCHWARZSCHILD (Miss E. M.), 184 Willesden Lane, London, N.W. 6.
SCOTT (Prof. J. W.), Windylaw, Lisvane, Cardiff.
SELIGMAN (P.), 27 Park Drive, Rayners Lane, Harrow, Middlesex.
SHIRLEY (A. W.), St. David's College, Lampeter, Cardiganshire.
SINCLAIR (W. A.), 5 Great Stuart Street, Edinburgh 3. *Life Member*.
SINGER (Dr. M. G.), Sage School of Philosophy, Cornell University, Ithaca, N.Y., U.S.A.
SKEMP (J. B.), 12 Eslington Terrace, Newcastle-on-Tyne 2.
SMART (J. J. C.), Corpus Christi College, Oxford.
SMETHURST (Rev. Dr. A. F.), Flat 1, Bemerton Rectory, Salisbury, Wilts.
SMILEY (P. O. R.), Glyn Dyfi, Caradog Road, Aberystwyth, Cards.
SMITH (A. H.), The Warden's Lodgings, New College, Oxford.
SMITH (Prof. J. E.), Barnard College, Columbia University, New York 27, N.Y., U.S.A.
SMITH (Prof. N. K.), The University, Edinburgh.
SMITHERS (D. L.), Pembroke College, Oxford.
SPENCER BROWN (G.), Trinity College, Cambridge.
SPILSBURY (R. J.), University College, Aberystwyth, Wales.
SPRINKLE (Rev. H. C.), World Outlook, 150 Fifth Ave., New York 11, N.Y., U.S.A.
STACE (Prof. W. T.), The University, Princeton, N.J., U.S.A.
STANGER (P. W.), 85 Potters Road, New Barnet, Herts.
STAPLEDON (W. O.), 7 Grosvenor Avenue, West Kirby, Cheshire.
STEPHENS (Prof. R. G.), 821 So. High St., Bloomington, Ind., U.S.A.
STEVENSON (Prof. C. L.), 904 Olivia Avenue, Ann Arbor, Mich., U.S.A.
STOB (H. J.), 1334 Sherman Street, Grand Rapids, Mich., U.S.A.
STORER (Prof. T.), Department of Philosophy, University of Nebraska, Lincoln 8, Neb., U.S.A.
STOUT (Prof. A. K.), The University, Sydney, Australia.
STRAWSON (P.), University College, Oxford.
STRETCH (W.), 13 Highfield Road, Bramhall, Stockport.
SULLIVAN (D. H. H.), Brackley Way House, Towcester, Northants.
SUTCLIFFE (Rev. T. H.), 8 Victoria St., Haslingden, Rossendale, Lancs.

TAYLOR (Pres. H.), Sarah Lawrence College, Bronxville, N.Y., U.S.A.
TEN HOOR (Dean M.), 4 Forest Lake Drive, Tuscaloosa, Ala., U.S.A.
TENNEY (Prof. C. D.), Southern Illinois University, Carbondale, Ill., U.S.A.
THALHEIMER (Dr. A.), 5603 Roxbury Place, Baltimore 9, Md., U.S.A.
THOMAS (Prof. G. F.), Elm Road, Princeton, N.J., U.S.A.
THOMAS (L. E.), Glyn Cottage, Belmont Road, Bangor, Caernarvonshire.
THOMPSON (Miss C. M.), 5 Halden Terrace, Dawlish, Devon.
THOMPSON (W. M. D.), Island of Stronsay, Orkney, Scotland.
THOMSON (R.), University College, The Castle, Durham.

Tomas (Prof. V.), Department of Philosophy, Brown University, Providence 12, R.I., U.S.A.
Toms (E.), 30 Kingsborough Gardens, Glasgow, W. 2.
Toulmin (S. E.), 3 Warnborough Road, Oxford.
Tree (Rev. R. J.), St. David's College, Lampeter, Cardiganshire.
Tulloch (Miss D. M.), Home Lodge, Broughty Ferry, Angus.
Turner (J. P.), 430 West 116th Street, New York 27, N.Y., U.S.A.
Turquette (Prof. A. R.), 322 Gregory Hall, University of Illinois, Urbana, Ill., U.S.A.

Urmson (J. O.), Christ Church, Oxford.

Vallance (D.), 23 Deepdene Road, Westerton, by Glasgow.
Van den Haag (E. M.), 58 Morton Street, New York 14, N.Y., U.S.A.
Varley (H.), 2 Fairfax Avenue, Didsbury, Manchester 20.
Veatch (Prof. H.), Department of Philosophy, Indiana University, Bloomington, Ind., U.S.A.
Vesterling (A. W.), 12 Ventnor Villas, Hove, Sussex.
Vinelott (J. E.), Queen's College, Cambridge.
Virpsha (S./Ldr. E. S.), Royal Automobile Club, London, S.W. 1.

Walkden (H.), The Raft, Derbyshire Road, Sale, Manchester.
Walker (Rev. L. J.), S.J., Campion Hall, Oxford.
Walley (J. T.), Chardleigh Green House, Chard, Somerset.
Walsh (W. H.), Merton College, Oxford.
Walters (R. H.), Auckland University College, Auckland C. 1., New Zealand.
Waterhouse (Prof. E. S.), The College Villa, Richmond Hill, Surrey.
Watkins (J. W. N.), Room 1816, Silliman College, Yale University, New Haven, Conn., U.S.A.
Watts (Dr. A. F.), 4 Sussex Square, Brighton 7.
Webb (Prof. C. C. J.), The Old Rectory, Pitchcott, Aylesbury, Bucks.
Weber (A. O.), 252 First Street, Osawatomie, Kan., U.S.A.
Webster (D.), Idlewild, Fountainhall Road, Aberdeen.
Weitz (Prof. M.), Vassar College, Poughkeepsie, N.Y., U.S.A.
Weldon (T. D.), Magdalen College, Oxford.
Werkmeister (Prof. W. H.), University of Nebraska, Lincoln 8, Neb., U.S.A.
Wernham (A. G.), 19 College Street, St. Andrews, Scotland.
White (Prof. M. G.), Emerson Hall, Harvard University, Cambridge, Mass., U.S.A.
Whitehead (Ven. L. G.), All Saints Vicarage, 786 Cumberland Street, Dunedin, N.Z.
Whitehouse (S. P.), The Parsonage, Dukinfield, Cheshire. *Life Member.*
Whiteley (C. H.), 42 Sandford Road, Birmingham 13.
Whitrow (G. J.), 23 Cedars Road, Clapham, London, S.W.4.
Wick (Prof. W. A.), Department of Philosophy, University of Chicago, Chicago 37, Ill., U.S.A.
Widgery (Prof. A. G.), Duke University, Durham, N.C., U.S.A.
Wieman (Prof. H. N.), Star Route 76512, Wrightwood, Cal., U.S.A.
Wienpahl (Prof. P. D.), Santa Barbara College, Santa Barbara, Cal., U.S.A.
Wightman (S.), Danby House, Harthill, near Sheffield.
Williams (Prof. D. C.), Emerson Hall, Harvard University, Cambridge, Mass., U.S.A.
Wisdom (J.), Trinity College, Cambridge.
Wisdom (J. O.), The London School of Economics, London, W.C. 2.
Wolfenden (J. F.), Kingsland House, The Schools, Shrewsbury.
Wolfson (Prof. H. A.), Widener 45, Harvard University, Cambridge 38, Mass., U.S.A.
Wolters (Mrs. G.), 45 Albert Road, Caversham, Reading.
Wood (O. T.), 14 Hillcroft Crescent, Ealing, London, W. 5.
Woods (R. G.), 10 Springfield St., Market Harborough, Leics.